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AND

THE ARTS.

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ARTICLE I.

Abstract of a Memoir on Camphor and the Camphoric Acid, read to the First Class of the National Institute of France. By BOUILLON LA GRANGE *.

CAMPHOR, of which the relation, the co-existence, and almost the identity with the volatile oils, have long since fixed the attention of chemists, has appeared to me to deserve a particular examination. I was desirous of confirming the discovery of Mr. Kosegarten on the property of camphor to become converted into a peculiar acid by the action of nitrous acid. The enquiries to which this undertaking has led me, have not only confirmed his discovery, but have likewise presented various facts of immediate interest to the vegetable analysis.

Camphor has long been an object of chemical attention. Several chemists have supposed that it exists in all the aromatic plants, particularly those which contain abundance of volatile oil; but as the camphor obtained from most of these plants constantly retains the odour of the plant which afforded it, there is reason to think, according to the remark of Citizen Fourcroy, that it is combined with the volatile oils of these vegetables. I hope to clear up this fact as soon as I shall procure a sufficient quantity of recent volatile oils; for my experiments have shewn that these afford the greatest quantity.

Proust, who has made experiments relative to this object, has merely indicated a process

* Annales de Chimie xxiii. 153.

for obtaining the camphor of ſeveral volatile oils; but he has neither deſcribed its character, nor ſhewn in what reſpect it might differ from ordinary camphor.

The experiments we are about to deſcribe may perhaps hereafter afford ſome light on the nature of this ſingular ſubſtance.

We know that the earths, the ſaline earthy ſubſtances, and the alkalis, have no action upon camphor; but as chemiſts had hitherto employed the alkalis in combination with carbonic acid, it was eſſentially neceſſary to try the action of the pure alkalis.

I ſhall not here enlarge upon the various experiments I have made. It will be ſufficient to remark, that I have obtained no ſatisfactory reſult. The camphor is always volatilized. It ſeems as if a ſmall quantity was diſſolved; but it is ſenſible only to the ſmell, for I have not been able to aſcertain its preſence by any other means.

The acids diſſolve camphor. The nitric acid is moſt commonly employed for this purpoſe, to form a medical preparation known by the improper name of oil of camphor.

I ſhall not attempt to explain the inconveniencies and injudicious method of preparing this oil. It is known that it is impoſſible to uſe it in any vehicle whatever without regenerating the camphor; conſequently there remained to medical practitioners no other means but of employing this oil alone, or the camphor united to alcohol. But if we examine the action of this oil on the animal economy, it will be immediately ſeen that it cannot be exhibited alone.

Theſe conſiderations engaged me in the ſearch for the means of obtaining camphor in the fluid ſtate, without the addition of an acid. Though the reſult of my experiments has not been ſucceſsful, it will nevertheleſs be uſeful to ſcience in fixing our notions reſpecting the nature of this ſubſtance.

SECTION II.

Proceſſes for obtaining the Decompoſition of Camphor.

TAKE one part of camphor and ſix of clay. Let the clay be pulverized in the dry ſtate, and ſifted through a fine ſieve; on the other hand, the camphor is to be pulverized by means of alcohol, and the whole is then to be mixed together in a mortar. When the mixture is well made, add a ſmall quantity of water, not too much, but enough to admit of forming the matter into a paſte, which is to be made into balls of the ſize of an olive. Place theſe on a hair ſieve to dry in a ſtove.

When the balls are perfectly dry, let them be introduced into a retort, which is to be placed on a ſand bath, with a receiver containing ſome diſtilled water, and well luted at the neck. In this diſpoſition of the apparatus the retort muſt be gradually heated, and kept for ſeveral hours in this ſtate, after which the heat is to be raiſed.

If the heat be not well managed, it ſometimes happens that part of the camphor ſublimes to the neck of the retort. This however is no reaſon for diſcontinuing the proceſs, which muſt be carried on ſo long as any oil is ſeen to come over. The fire is then to be put out; and when the apparatus is cold, the receiver may be unluted, and the ſublimed camphor, as well as the reſidue in the retort, muſt be taken out. The whole is then to be pounded in a mortar, and the operation recommenced as before, until, even by a conſiderable degree of heat, neither ſublimation nor oil appears.

A ſecond

A second Process.

The foregoing operation may be conducted in another manner, namely by reducing the two ſubſtances into a very fine powder, and introducing them into the retort.

This method, though apparently more ſpeedy, is in fact much leſs ſo than the foregoing; for the camphor riſes with much more facility, and the diſtillation muſt be oftener repeated, which renders the operation much more tedious.

Third Proceſs.

Alumine is likewiſe a medium which may be uſed with advantage.

The alumine is prepared by means of ammoniac; and it is afterwards waſhed with diſtilled water, in order to obtain it as pure as poſſible. It is then ſlightly dried.

Take two parts of alumine, or three, when it is wetted, to one part of camphor. The camphor is firſt to be pulverized, and the alumine afterwards added. The whole may be formed into a paſte with water, or otherwiſe the pulverulent mixture is to be introduced into the retort; but I prefer the firſt method, as I have already remarked. A receiver containing ſome diſtilled water is then to be adjuſted. The pneumatic apparatus may alſo be adapted. Diſtillation is then to be made by a gentle heat.

By either of theſe proceſſes a volatile oil of a golden yellow colour comes over into the receiver, and floats at the ſurface of the water. We ſhall proceed to deſcribe its characters.

The diſtilled water placed in the receiver is not only neceſſary to prevent the loſs of oil, which would attach itſelf to the interior ſurface, but likewiſe to abſorb a ſmall portion of camphoric acid which is formed during the operation. In fact this water is very aromatic and penetrating: it reddens the tincture of turnſol, and effervеſces with the alkaline carbonates. The acid itſelf, at the end of a certain time, cryſtallizes on the ſides of the veſſel.

After the operation is completed, the matter remaining in the retort is of a very deep black colour.

Four ounces of camphor, or 122,284 grammes, may produce

Volatile oil 12 gros, or 45,856 grammes;

Carbone 8 gros, or 30,571 grammes;

Carbonated hydrogen gas; carbonic acid; camphoric acid, each in proportions which I have not been able to determine, on account of the volatility of the camphor.

SECTION III.

An Examination of the Oil obtained from Camphor in the foregoing Experiments.

THE oils obtained by the different proceſſes have no difference in their qualities.

This oil has an acrid burning taſte, and leaves a perception of coolneſs on the tongue. Its ſmell is aromatic, reſembling that of thyme or roſemary. Its colour golden yellow. When expoſed to the open air it partly evaporates, and there remains a brown thick yellow matter of a very ſharp taſte, but ſomewhat more bitter, which at length entirely evaporates.

With the cauſtic alkalis it affords an homogenous mixture, ſoluble in water, and which has all the characters of ſoaps made with volatile oils.

Alcohol diſſolves it entirely; and if diſtilled water be added to the ſolution, the mixture becomes milky, but affords no precipitate.

When brought into contact with the oxigenated muriatic acid, this oil becomes very white, but no precipitate appears.

It therefore differs from that obtained by the nitric acid; becauſe, when mixed with water, with alkalis, or with the oxigenated muriatic acid, camphor is regenerated.

If the muriatic acid gas be paſſed through the oil of camphor prepared by the nitric acid, its colour immediately becomes of a roſe red, and the inſtant afterwards yellow. This laſt colour does not vary until towards the end of the operation.

The oil itſelf does not acquire new properties.

SECTION IV.

Examination of the Matter found in the Retort.

THE matter which remains in the retort after the operation, is, as we have remarked, of a beautiful black, and, when examined as to quantity, it is found to weigh more than the alumine alone.

It conſiſts of carbone very accurately mixed with the alumine, and accordingly the powder is extremely fine.

In order to ſeparate the carbone from the alumine, and leave it in a ſtate of purity, the alumine muſt be ſaturated with an acid. For this purpoſe the ſulphuric acid is to be applied in the diluted ſtate. This acid diſſolves the alumine, and leaves the carbone diſengaged, which may be ſeparated and dried upon the filtre. The ſulphat of alumine may be obtained by concentrating the fluid in cryſtals, if a ſmall portion of alkali be added.

The intimate combination of the carbone with alumine led me to ſuſpect that this method might probably be advantageous to obtain the carburet of alumine, a preparation hitherto attempted without ſucceſs, and conſequently unknown to chemiſts.

I therefore thought it neceſſary to repeat the experiment.

I took an indeterminate quantity of this kind of carburet of alumine, which I placed under a glaſs veſſel filled with diſtilled water, which veſſel was inverted over another veſſel filled with water.

At the end of a certain time bubbles of air were diſengaged, which roſe to the upper part of the veſſel, and gradually diſappeared. The water in the veſſel became depreſſed, and was replaced by two gaſes, one of which had all the characters of the carbonic acid gas, and the other was inflammable, and proved to be hydrogen.

This experiment conſequently proves, that a ſmall portion of the water was decompoſed, its oxigen having ſeized the carbone, and formed carbonic acid, while the hydrogen became diſengaged.

Notwithſtanding the accuracy of this experiment, it is inſufficient to prove that an intimate combination had taken place; or, in a word, that a true carburet of alumine had been formed. Neverthele ſs, until experiments ſhall have eſtabliſhed the characters which ought

ought to diſtinguiſh an earthy carburet, particularly that of alumine, I ſhall apply this name to diſtinguiſh the reſult of the new product obtained by the decompoſition of camphor.

We cannot have any further doubt reſpecting the exiſtence of carbone in camphor; but we ſhould render this object much more intereſting, if we could prove our aſſertion by ſyntheſis. Vegetables do not, however, in all caſes, permit us to imitate Nature in her proceſſes. It frequently happens, that the beſt products of Art are but imperfect approximations to natural compounds.

I do not, however, deſpair of our forming camphor hereafter by means of a volatile oil. Various eſſays on the contrary allow me to aſſert that it is poſſible to effect this.

A few experiments which follow will render this aſſertion probable.

SECTION V.

Proceſſes for carbonizing a Volatile Oil.

ONE ounce, or 30,571 grammes of carburet of pot-aſh, or pot-aſh fuſed with charcoal, and 4 ounces, or 122,284 grammes of volatile oil of turpentine, or other volatile oil, were taken. Theſe two ſubſtances were put into a matraſs, and the whole was macerated by a gentle heat. The oil acquired a very deep black colour; its ſmell was changed and became more aromatic. If in theſe experiments the oil thus changed be poured into diſtilled water, the fluid becomes white, and a precipitate is formed. If the liquor be left in contact with the atmoſpheric air, a few drops of oil riſe and float at the ſurface, but the liquor continues white. From a perſuaſion that this combination had formed a ſaponule, I ſaturated the pot-aſh with an acid, and no true oil was ſeparated, but it remained ſuſpended in the water, which was become opaque and white.

As the addition of pot-aſh rendered my experiment uncertain, I repeated it in another manner.

By the advice of Cit. Guyton, I paſſed hydrogen gas (obtained by the action of ſulphuric acid upon zinc) through oil of turpentine, into which I had put the powder of charcoal. I was careful to keep the oil conſtantly hot, in order to promote the ſolution of the coal by the hydrogen gas.

In proportion as the gas was diſengaged, the fluid became deeper coloured; and, inſtead of continuing limpid, it became yellow, and at laſt brown. If the pneumato-chemical apparatus be adapted to the veſſel containing the oil, hydrogen gas is collected together with a ſmall portion of carbonic acid.

The oil by this treatment has therefore taken up a ſmall quantity of carbone and hydrogen, as appears by the difference between its properties and thoſe of pure oil; viz. 1. It is deeper coloured: 2. Its ſmell is more agreeable, *(ſuave)*: 3. And, when mixed with water, this fluid becomes turbid, and the oil does not ſeparate but by expoſure to the air, which leaves the water conſtantly opaque.

It muſt be admitted that theſe trials afford no ſatisfactory reſult. But I have no doubt but that a continued ſet of experiments on this object would afford decided concluſions, or at leaſt ſome elucidations of utility to the progreſs of ſcience.

[The remainder in our next.]

II. *In-*

II.

Instructions concerning the Manufacture of Steel, and its Uses. By VANDERMONDE, MONGE, *and* BERTHOLLET. *Published by Order of the Committee of Public Safety.*

[Concluded from Vol. ii. p. 70.]

Of Cast Steel.

CAST Steel is produced by fusion of natural steel, particularly that of cementation. The fluid state assumed by the metal in this operation causes the flaws and veins to disappear, and renders the whole mass more uniform.

According to the description which Jars has given us of the manner in which this operation is performed at Sheffield, all kinds of fragments of broken steel are used. The furnace is of the same kind as that of the brass-founder, but much smaller, and supplied with air by a subterraneous communication. At the mouth of the furnace, which is square, and level with the earth, there is an opening against a wall where a chimney is carried up. These furnaces contain only one large crucible nine or ten inches high, and six or seven in diameter. The steel is put into the crucible with a flux, the composition of which is kept secret: and the crucible itself is placed on a round brick standing on the grate. Coak is placed round the crucible, and the upper part of the furnace is filled with it. It is then set on fire; and the upper opening of the furnace is entirely closed by a covering formed of bricks, bound together with iron.

The crucible remains five hours in the furnace before the steel is perfectly fused. Several operations are afterwards made. Moulds formed of two pieces of cast iron, which fit together, and form an octagonal or square cavity, are prepared for casting the steel, which is afterwards hammered out in the same manner as blister-steel, but with less heat and more care, because of the danger of breaking it.

Chalut, officer of artillery, has made experiments on the flux which is best adapted for making cast steel. He is convinced that every kind of glass may be used as a flux, except that which contains lead or arsenic.

The steel being broken into small pieces, is to be covered with the glass. The cover of the crucible must then be put on, and the heat urged to the greatest degree of the brass-founders furnace.

It appears that an extraordinary hardness is sometimes required to be given to cast steel, and that this effect is produced by mixing coaly matter with the flux, to saturate the steel, and give it the highest degree of hardness. It is probable that certain instruments are manufactured by some process of this kind; such as cylinders and laminating rollers, of which the hardness is very great, and the grain perfectly uniform through the whole mass:—but on this subject we have nothing to offer but conjectures.

One of the greatest difficulties we find in this country (France) in the fusion of steel, is to procure good crucibles. The art of pottery, which is truly important in every one of its parts, is that which, of all others, the most strongly solicits our industry.

Concerning the peculiar Properties of the different Kinds of Steel.

CAST steel may be considered as the most perfect for all such instruments as require a beautiful polish and uniform hardness. It is exempt from the flaws, dull spots, and veins, which are more or less abundant in other steel. This steel ought to be chosen for such tools as require to be hard and well polished; such as burnishers, watchmakers' tools, lancets, razors, and ornamental steel-work; but it has the inconvenience of not being welded* with iron, and is brittle. It is more difficult to treat in the fire, and is necessarily much dearer than common steel, because it is obtained by fusion of that very article.

This kind of steel is highly valuable to the arts which promote luxury; but we must direct our attention to the steel which forms the hatchet, the file, the sabre, and the hammers of gun-locks.

Steel of cementation sometimes approaches to cast steel in purity, when excellent iron has been used, and particularly when it has been well forged; but in general it presents some threads and veins, is less uniform in its texture, and does not become equally hard. This steel may be used for most articles of cutlery, edge-tools, laminating rollers, hammers, small springs, files, &c. It is not easily welded to iron.

This steel is not only used for a great variety of objects, but it may be used in different proportions in the stuffs (Etoffes†) which are used when a material is required which shall be less subject to break, as in the fabrication of large springs, scythes, sabres, &c.

The material called stuff (Etoffe) is a compound of iron and steel, several plates of which are forged and welded together, to afford a substance partaking of the properties of both. The iron seems to give flexibility to the steel, while this communicates its hardness and elasticity to the iron. It appears that the art of uniting plates of steel and iron, and of contorting them together, is the principal requisite in the perfection of Damascus steel.

Natural steel is much less uniform than the steel of cementation. When it is polished, its surface is usually rendered unequal by scabrous places, flaws or veins. It is easy to discover veins of iron with the point of the graver, so that it may be considered as a natural composition; whence it is found, that cutting tools made of this steel are less subject to fail in the edge, and that it bears heating a second time very well; having, as the workmen say, more body, and being more easy to forge.

In general, according to our eminent cutler Perret, it is adviseable to use the English cemented steel for fine and delicate work, and sometimes even cast steel; but for strong and heavy work, the preference is to be given to German steel, which is the natural steel, because it has more body and tenacity.

Upon the whole, it appears to us that the steel of cementation is most worthy of being attended to; because it is easy, at a small charge, to establish manufactories on a confined scale, and with the desired expedition; and because this steel may, without difficulty, be used in any kind of work. Fellow citizens, let us convey to our forges those expensive balustrades and railing, which have nothing to defend; and if we find them to possess the qualities of good iron, let us convert them into steel.

* For the method of welding cast-steel to iron, see Philos. Jour. I. 575. N.

† I am not aquainted with the appropriate English term. N.

The

The Method of trying Steel.

THE different properties belonging to each kind of ſteel render the proofs of their quality difficult even to ſkilful workmen. Caſt ſteel will be thought of a very bad quality by him who is not in the habit of working it; as will likewiſe the ſteel of cementation by the workman who is commonly employed upon German ſteel. The grain of the fracture is a deceitful indication, becauſe its fineneſs varies with its hardneſs; nevertheleſs good ſteel ought always to exhibit an even grain. Caſt ſteel ought to take a good poliſh, and not be too brittle: ſteel of cementation ought to afford gravers capable of reſiſting percuſſion without notching or turning in the edge; natural ſteel ought to weld with eaſe to iron, and make good edge-tools.

There are circumſtances in which it is of advantage to aſcertain whether a manufactured article be formed of ſteel or iron without injuring it. The means of doing this is of the more conſequence to us, becauſe certain unprincipled contractors have delivered ſabres with blades of pure iron, to which a ſlight degree of rigidity had been given in the fabrication; a circumſtance which has induced the Committee of Public Safety to publiſh the directions we ſhall here tranſcribe, and to oblige its agents, entruſted with the reception of every kind of arms of ſteel, to ſubmit them to this proof.

"If a drop of nitrous acid be placed upon the ſurface of poliſhed iron, and, after having left it two minutes, water be thrown thereon, this laſt fluid will carry off the acid with all its contents, and there will remain merely a white ſpot, or the colour of iron newly cleaned.

"If the ſame operation be made on a plate of poliſhed ſteel, the acid likewiſe ſeizes the iron, but does not act on the coaly part. This laſt falls down during the ſolution, and forms a black ſpot, which the projection of the water does not remove, and which even remains for a conſiderable time, on account of its adheſion.

"In order to ſucceed in this operation, a diluted acid muſt be uſed, becauſe the coaly principle does not adhere to the ſurface, but in proportion to the ſlowneſs of the ſolution, and the weakneſs of the effervеſcence.

"If pure or rectified nitrous acid be not at hand, the aquafortis of commerce may be uſed, taking care to dilute it in a certain degree.

"The drop of acid muſt be conveyed with a ſtick of glaſs, or other material, which is not itſelf attacked, and cannot afford any thing capable of changing the reſult.

"The ſmalleſt drop ſuffices. It ought rather to be ſpread out, than kept together, that it may mark a larger ſurface. The ſtopper of a very ſmall bottle, in which the acid is kept, anſwers this purpoſe very well.

"After having made this proof two or three times on iron and ſteel compared with each other, the operator will be enabled to ſpeak with certainty reſpecting the difference of the metals."

Artiſts have long ſince availed themſelves of a method of this kind to diſtinguiſh Damaſcus blades. Theſe blades, as we have obſerved, are compoſed of the ſteel and iron intimately mixed. By this proof, ſays Perret, they preſent ſerpentine veins, ſome of a whitiſh grey, others of a deep grey, and others blackiſh, which are known by the name of the Flowers of Damaſcus.

We

We have remarked that cast iron, sufficiently abounding with charcoal, assumes the appearance of the white crude iron when it is cast in plates or too suddenly cooled. To ascertain its nature, nothing more is necessary than to clean its surface; and by the more or less grey or dark colour of the spot produced by nitrous acid, a judgment may be formed of its quality.

VANDERMONDE, MONGE, BERTHOLLET.

Annotations upon the Report concerning Steel.

1. NOTWITHSTANDING the great light which has been afforded by the labours of Rinman, Bergman, Vauquelin, and other chemists, together with the learned authors of the foregoing paper, we have still much to learn respecting the causes of the several qualities of steel to be found in the market. Much of this information might probably be obtained by diligent enquiry, and the usual methods of examination. It is yet to be shewn by experiment, whether pure iron united with carbon constitutes the best steel; or whether any additional ingredient, such as phosphorus, manganese, or other metallic matter, may not be of advantage. If the former position be true, the best steel-iron will be that which is the purest; and the best cement will be the simplest coaly matter. On the contrary, if the latter suppositions should be well founded, the goodness of our steel will depend, not only upon the component parts of the bar iron, but likewise on the adaptation of the cement proper for its conversion. Mr. Duhamel, in the Encyclopédie, article *Acier*, p. 462, found that the charcoal which had been used in making steel was not fit to be used again, by reason of the slowness of its operation. In this case it seems probable that, though enough of carbone remained, yet some more volatile ingredient had been expended in the first process. Our authors have well explained the advantage of the iron being well forged and sound throughout, previous to its exposure to the cementing process.

2. The grey crude iron may be considered as iron saturated with as much plumbago or carburet as it can hold in solution at a very elevated temperature. When it is suffered to cool slowly, as in soft casting, the plumbago appears to separate by hasty crystallization through the whole mass, as may frequently be observed in its fracture, and as has been shewn by plunging a cold bar of iron into the fused metal, and withdrawing it covered with the carburet which precipitates upon it. When this iron is poured into a cold mould of metal, or suffered to run to a great distance from the aperture of reception, in sand, or otherwise if a bar of this metal be heated to whiteness, and plunged in water, it becomes very hard; more so than steel tools of any description are usually made, and is very white, and closer grained in its fracture. The hardness must depend upon circumstances in its aggregation, to explain which we possess no data; but I apprehend the whiteness to be an evident consequence of the union between the iron and the plumbago continuing to be nearly as intimate as it was at the white heat. It is very probable that, in all cases of the hardening of other steels containing less carbone, a similar effect may take place.

3. The laminating rollers, concerning which our authors offer nothing but conjecture, are made of this iron cast in sand, or metallic moulds of considerable thickness. I am informed by founders who make them, that the external hard part does not penetrate to a greater depth than about three quarters of an inch; and that the hardness is not greater than that of a good graver. The process of turning them in the lathe, in fact, shews this.

For they are turned by an extremely ſlow motion, with a tool ſelected from the general ſtock in the work-ſhop, becauſe the greater number even of good gravers will not cut it.

4. Thoſe who forge ſteel know that it is very eaſily degraded in the fire. If a ſmall piece, for example, half a cubic inch, of grey crude iron be put into a common fire, and kept red-hot for about half an hour, and at the expiration of that time the heat be ſuddenly raiſed to whiteneſs by the bellows; the internal or ſteely part will break its way through the external cruſt, which is converted into common iron, and rendered much leſs fuſible. This cruſt, or hollow ſhell, may then be taken out of the fire. All ſteel becomes degraded in the ſame manner, even by very careful heating. Caſt-ſteel, drawn into ſmall bars, exhibits cloudy lines and veins on its ſurface when tried by an acid, which no doubt have been produced during the heating and forging.

5. Caſt-ſteel being made out of broken tools of every kind, cannot of itſelf poſſeſs a larger doſe of plumbago than the average quantity contained in thoſe ſteels. But the Engliſh caſt-ſteel is more fuſible and more tender under the hammer than German ſteel, or the ſteel of cementation; which circumſtances appear to indicate that it contains more plumbago: and the truth of this induction is confirmed by its exhibiting a much darker ſpot than other ſteels, when tried by an acid. Chalut did not therefore make this kind of ſteel when he uſed glaſs only for his flux. It cannot be doubted but that the flux of our manufacturers muſt contain charcoal, at leaſt. If it be animal coal, which is moſt probable, it will alſo contain phoſphorus; an ingredient to which the ſuperiority of this coal, in caſe-hardening, is probably owing.

6. Tenacity and hardneſs are very frequently conſidered as if they were one and the ſame quality with regard to implements and tools, though they are certainly very diſtinct properties. Tenacity is the oppoſite to friability or brittleneſs; hardneſs is the oppoſite to ſoftneſs. It is probable that iron is more tenacious than ſteel, and it is certain that ſoft ſteel is more tenacious than hard. Where tenacity and no conſiderable degree of hardneſs are wanting, as in ſprings, the inferior ſteels, or the compounds of ſteel and iron, will afford a more ſafe, and conſequently in many inſtances more uſeful article; but where hardneſs and tenacity are both required, the leading quality of the ſteel muſt be its uniformity. Caſt ſteel is preferred in England to every other kind, not only for poliſhed ſteel-work and the beſt cutting tools, but likewiſe for cold chiſels, and the hard gravers for turners in metal; in both which laſt I have obſerved, by conſiderable experience, that the common opinion is well founded.

III.

An Enquiry concerning the Source of the Heat which is excited by Friction. By* BENJAMIN, COUNT *of* RUMFORD, *F.R.S. M.R.I.A.*

IT frequently happens that, in the ordinary affairs and occupations of life, opportunities preſent themſelves of contemplating ſome of the moſt curious operations of nature; and

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very

very interefting philofophical experiments might often be made, almoft without trouble or expence, by means of machinery contrived for the mere mechanical purpofes of the arts and manufactures.

I have frequently had occafion to make this obfervation; and am perfuaded, that a habit of keeping the eyes open to every thing that is going on in the ordinary courfe of the bufinefs of life, has oftener led, as it were by accident, or, in the playful excurfions of the imagination put into action by contemplating the moft common appearances, to ufeful doubts and ferviceable fchemes for inveftigation and improvement, than all the more intenfe meditations of philofophers in the hours exprefsly fet apart for ftudy.

It was by accident that I was led to make the experiments of which I am about to give an account; and though they are not, perhaps, of fufficient importance to merit fo formal an introduction, I cannot help flattering myfelf that they will be thought curious in feveral refpects, and worthy of the honour of being made known to the Royal Society.

Being engaged, lately, in fuperintending the boring of cannon in the workfhops of the military arfenal at Munich, I was ftruck with the very confiderable degree of heat which a brafs gun acquires in a fhort time in being bored, and with the more intenfe heat (much greater than that of boiling water, as I found by experiment) of the metallic chips feparated from it by the borer.

The more I meditated on thefe phenomena, the more they appeared to me to be curious and interefting. A thorough inveftigation of them feemed even to bid fair to give a farther infight into the hidden nature of heat; and to enable us to form fome reafonable conjectures refpecting the exiftence or non-exiftence of an *igneous fluid*: a fubject on which the opinions of philofophers have in all ages been much divided.

In order that the Society may have clear and diftinct ideas of the fpeculations and reafonings to which thefe appearances gave rife in my mind, and alfo of the fpecific objects of philofophical inveftigation they fuggefted to me, I muft beg leave to ftate them at fome length, and in fuch manner as I fhall think beft fuited to anfwer this purpofe.

From *whence comes* the heat actually produced in the mechanical operation above mentioned?

Is it furnifhed by the metallic chips, which are feparated by the borer from the folid mafs of metal?

If this were the cafe, then, according to the modern doctrines of latent heat and of caloric, the *capacity for heat* of the parts of the metal fo reduced to chips, ought not only to be changed, but the change undergone by them fhould be fufficiently great to account for *all* the heat produced.

But no fuch change had taken place; for I found, upon taking equal quantities by weight of thefe chips, and of thin flips of the fame block of metal, feparated by means of a fine faw, and putting them at the fame temperature (that of boiling water) into equal quantities of cold water (that is to fay, at the temperature of $59\frac{1}{2}°$ F.), the portion of water into which the chips were put was not, to all appearance, heated either lefs or more than the other portion into which the flips of metal were put.

This experiment being repeated feveral times, the refults were always fo nearly the fame, that

that I could not determine whether any, or what change had been produced in the metal, *in regard to its capacity for heat*, by being reduced to chips by the borer*.

From hence it is evident, that the heat produced could not possibly have been furnished at the expence of the latent heat of the metallic chips. But, not being willing to rest satisfied with these trials, however conclusive they appeared to me to be, I had recourse to the following still more decisive experiment.

Taking a cannon (a brass six pounder) cast solid, and rough as it came from the foundry, (see fig. 1, plate V) and fixing it (horizontally) in the machine used for boring, and at the same time finishing the outside of the cannon by turning, (see fig. 2.) I caused its extremity to be cut off, and, by turning down the metal in that part, a solid cylinder was formed, $7\frac{3}{4}$ inches in diameter, and $9\frac{8}{10}$ inches long; which, when finished, remained joined to the rest of the metal (that which, properly speaking, constituted the cannon,) by a small cylindrical neck, only $2\frac{1}{5}$ inches in diameter, and $3\frac{8}{10}$ inches long.

This short cylinder, which was supported in its horizontal position, and turned round its axis, by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer used in boring cannon; but its bore, which was 3.7 inches in diameter, instead of being continued through its whole length, (9.8 inches) was only 7.2 inches in length; so that a solid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickness.

This cavity is represented by dotted lines in fig. 2; as also in fig. 3, where the cylinder is represented on an enlarged scale.

This cylinder being designed for the express purpose of generating heat by friction, by having a blunt borer forced against its solid bottom at the same time that it should be turned round its axis by the force of horses, in order that the heat accumulated in the cylinder might from time to time be measured, a small round hole, (see d, e, fig. 3.) 0.37 of an inch only in diameter, and 4.2 inches in depth, for the purpose of introducing a small cylindrical mercurial thermometer, was made in it, on one side, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the solid part of the metal which formed the bottom of its bore.

* As these experiments are important, it may, perhaps, be agreeable to the Society to be made acquainted with them in their details. One of them was as follows:

To 4590 grains of water, at the temperature of $59\frac{1}{2}$° F. (an allowance or compensation, reckoned in water, for the capacity for heat of the containing cylindrical tin vessel, being included) were added $1016\frac{1}{8}$ grains of gun-metal in thin slips, separated from the gun by means of a fine saw, being at the temperature of 210° F. When they had remained together 1 minute, and had been well stirred about by means of a small rod of light wood, the heat of the mixture was found to be = 63°.

From this experiment, the specific heat of the metal, calculated according to the rule given by Dr. Crawford, turns out to be = 0.1100, that of water being = 1.000.

An experiment was afterwards made with the metallic chips, as follows:

To the same quantity of water as was used in the experiment above mentioned, at the same temperature, (viz. $59\frac{1}{2}$°,) and in the same cylindrical tin vessel, were now put $1016\frac{1}{8}$ grains of metallic chips of gun-metal, bored out of the same gun from which the slips used in the foregoing experiment were taken, and at the same temperature (210°.) The heat of the mixture, at the end of 1 minute, was just 63°, as before; consequently the specific heat of these metallic chips was = 0.1100. Each of the above experiments was repeated three times, and always with nearly the same results.

The

The solid contents of this hollow cylinder, exclusive of the cylindrical neck by which it remained united to the cannon, were 385¾ cubic inches, English measure, and it weighed 113.13 lb. avoirdupois, as I found on weighing it at the end of the course of experiments made with it, and after it had been separated from the cannon with which, during the experiments, it remained connected *.

Experiment No. 1.

THIS experiment was made in order to ascertain how much heat was actually generated by friction, when a blunt steel borer being so forcibly shoved (by means of a strong screw) against the bottom of the bore of the cylinder, that the pressure against it was equal to the weight of about 10,000 lb. avoirdupois, the cylinder was turned round on its axis (by the force of horses) at the rate of about 32 times in a minute.

This machinery, as it was put together for the experiment, is represented by fig. 2. W is a strong horizontal iron bar connected with proper machinery carried round by horses, by means of which the cannon was made to turn round its axis.

To prevent, as far as possible, the loss of any part of the heat that was generated in the experiment, the cylinder was well covered up with a fit coating of thick and warm flannel, which was carefully wrapped round it, and defended it on every side from the cold air of the atmosphere. This covering is not represented in the drawing of the apparatus, fig. 2.

I ought to mention, that the borer was a flat piece of hardened steel, 0.63 of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely, 3½ inches. Its corners were rounded off at its end, so as to make it fit the hollow bottom of the bore; and it was firmly fastened to the iron bar (m), which kept it in its place. The area of the surface, by which its end was in contact with the bottom of the bore of the cylinder, was nearly 2⅓ inches. This borer, which is distinguished by the letter n, is represented in most of the figures.

At the beginning of the experiment, the temperature of the air in the shade, as also that of the cylinder, was just 60° F.

At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horses being stopped, a cylindrical mercurial thermometer, whose bulb was $\frac{32}{100}$ of an inch in diameter, and 3¼ inches in length, was introduced into the hole made to receive it, in the side of the cylinder; when the mercury rose instantly to 130°.

Though the heat could not be supposed to be quite equally distributed in every part of the cylinder, yet, as the length of the bulb of the thermometer was such that it extended

* For fear I should be suspected of prodigality in the prosecution of my philosophical researches, I think it necessary to inform the Society, that the cannon I made use of in this experiment was not sacrificed to it. The short hollow cylinder which was formed at the end of it, was turned out of a cylindrical mass of metal about two feet in length, projecting beyond the muzzle of the gun, called in the German language the verlorner kopf, (the head of the cannon to be thrown away,) and which is represented in fig. 1.

This additional projection, which is cut off before the gun is bored, is always cast with it, in order that, by means of the pressure of its weight on the metal in the lower part of the mould, during the time it is cooling, the gun may be the more compact in the neighbourhood of the muzzle, where, without this precaution, the metal would be apt to be porous or full of honeycombs.

from

from the axis of the cylinder to near its surface, the heat indicated by it could not be very different from that of the mean temperature of the cylinder; and it was on this account that a thermometer of that particular form was chosen for this experiment.

To see how fast the heat escaped out of the cylinder, (in order to be able to make a probable conjecture respecting the quantity given off by it during the time the heat generated by the friction was accumulating,) the machinery standing still, I suffered the thermometer to remain in its place near three quarters of an hour, observing and noting down, at small intervals of time, the height of the temperature indicated by it. Thus,

At the end of 4 minutes, the heat, as shown by the thermometer, was	126°
After 5 minutes, always reckoning from the first observation,	125
At the end of 7 minutes, — — —	123
12 — — — —	120
14 — — — —	119
16 — — — —	118
20 — — — —	116
24 — — — —	115
28 — — — —	114
31 — — — —	113
34 — — — —	112
$37\frac{1}{2}$ — — — —	111
And when 41 minutes had elapsed — —	110

Having taken away the borer, I now removed the metallic dust, or rather scaly matter, which had been detached from the bottom of the cylinder by the blunt steel borer, in this experiment, and, having carefully weighed it, I found its weight to be 837 grains Troy.

Is it possible that the very considerable quantity of heat that was produced in this experiment (a quantity which actually raised the temperature of above 113 lb. of gun-metal at least 70 degrees of Fahrenheit's thermometer, and which, of course, would have been capable of melting $6\frac{1}{2}$ lb. of ice, or of causing near 5 lb. of ice-cold water to boil,) could have been furnished by so inconsiderable a quantity of metallic dust? and this merely in consequence of a change of its capacity for heat?

As the weight of this dust (837 grains Troy) amounted to no more than $\frac{1}{948}$th part of that of the cylinder, it must have given off 948 degrees to raise the temperature of the cylinder 1 degree; and consequently it must have given off 66360 degrees of heat to have produced that of the experiment!

But, without insisting on the improbability of this supposition, we have only to recollect, that from the results of actual and decisive experiments, made for the express purpose of ascertaining that fact, the capacity for heat, of the metal of which great guns are cast, is not sensibly changed by being reduced to the form of metallic chips, in the operation of boring cannon; and there does not seem to be any reason to think that it can be much changed, if it be changed at all, in being reduced to much smaller pieces by means of a borer that is less sharp.

If the heat, or any conſiderable part of it, were produced in conſequence of a change in the capacity for heat of a part of the metal of the cylinder, as ſuch change could only be ſuperficial, the cylinder would by degrees be exhauſted, or the quantities of heat produced, in any given ſhort ſpace of time, would be found to diminiſh gradually in ſucceſſive experiments. To find out if this really happened or not, I repeated the laſt-mentioned experiment ſeveral times with the utmoſt care ; but I did not diſcover the ſmalleſt ſign of exhauſtion in the metal, notwithſtanding the large quantities of heat actually given off.

Finding ſo much reaſon to conclude, that the heat generated in theſe experiments, or excited, as I would rather chooſe to expreſs it, was not furniſhed at the expence of the latent heat or combined caloric of the metal, I puſhed my enquiries a ſtep farther, and endeavoured to find out whether the air did or did not contribute any thing in the generation of it.

Experiment No. 2.

AS the bore of the cylinder was cylindrical, and as the iron bar (m), to the end of which the blunt ſteel borer was fixed, was ſquare, the air had free acceſs to the inſide of the bore, and even to the bottom of it where the friction took place by which the heat was excited.

As neither the metallic chips produced in the ordinary courſe of the operation of boring braſs cannon, nor the finer ſcaly particles produced in the laſt-mentioned experiments by the friction of the blunt borer, ſhowed any ſigns of calcination, I did not ſee how the air could poſſibly have been the cauſe of the heat that was produced; but, in an inveſtigation of this kind, I thought that no pains ſhould be ſpared to clear away the rubbiſh, and leave the ſubject as naked and open to inſpection as poſſible.

In order by one deciſive experiment to determine whether the air of the atmoſphere had any part or not in the generation of the heat, I contrived to repeat the experiment under circumſtances in which *it was evidently impoſſible for it to produce any effect whatever*. By means of a piſton exactly fitted to the mouth of the bore of the cylinder, through the middle of which piſton the ſquare iron bar, to the end of which the blunt ſteel borer was fixed, paſſed in a ſquare hole made perfectly air-tight, the acceſs of the external air to the inſide of the bore of the cylinder was effectually prevented. In fig. 3. this piſton (*p*) is ſeen in its place : it is likewiſe ſhown in fig. 7. and 8.

I did not find, however, by this experiment, that the excluſion of the air diminiſhed in the ſmalleſt degree the quantity of heat excited by the friction.

There ſtill remained one doubt, which, though it appeared to me to be ſo ſlight as hardly to deſerve any attention, I was however deſirous to remove. The piſton which cloſed the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with ſo much nicety by means of collars of leather, and preſſed againſt it with ſo much force, that, notwithſtanding its being oiled, it occaſioned a conſiderable degree of friction when the hollow cylinder was turned round its axis. Was not the heat produced, or at leaſt ſome part of it occaſioned, by the friction of the piſton ? And as the external air had free acceſs to the extremity of the bore where it came in contact with the piſton, is it not poſſible that this air might have had ſome ſhare in the generation of the heat produced ?

Experiment No. 3.

A quadrangular oblong deal-box, (fee fig. 4.) water-tight, 11½ Englifh inches long, 9$\frac{4}{10}$ inches wide, and 9$\frac{6}{10}$ inches deep, (meafured in the clear,) being provided, with holes or flits in the middle of each of its ends juft large enough to receive the one, the fquare iron rod to the end of which the blunt fteel borer was faftened, the other, the fmall cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occafionally clofed above by a wooden cover or lid moving on hinges) was put into its place; that is to fay, when by means of the two vertical openings or flits in its two ends, (the upper parts of which openings were occafionally clofed by means of narrow pieces of wood fliding in vertical grooves,) the box *(g, h, i, k,* fig. 3.) was fixed to the machinery in fuch a manner that its bottom *(i, k,)* being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder; it is evident from the defcription, that the hollow metallic cylinder would occupy the middle of the box without touching it on either fide, (as it is reprefented in fig. 3.) and that on pouring water into the box and filling it to the brim, the cylinder would be completely covered and furrounded on every fide by that fluid. And farther, as the box was held faft by the ftrong fquare iron rod *(m)*, which paffed in a fquare hole in the centre of one of its ends, *(a,* fig. 4.) while the round or cylindrical neck, which joined the hollow cylinder to the end of the cannon, could turn round freely on its axis in the round hole in the centre of the other end of it, it is evident that the machinery could be put in motion without the leaft danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus.

Every thing being ready, I proceeded to make the experiment I had projected in the following manner:

The hollow cylinder having been previoufly cleaned out, and the infide of its bore wiped with a clean towel till it was quite dry, the fquare iron bar, with the blunt fteel-borer fixed to the end of it, was put into its place; the mouth of the bore of the cylinder being clofed at the fame time by means of the circular pifton, through the centre of which the iron bar paffed.

This being done, the box was put in its place; and the joinings of the iron rod and of the neck of the cylinder, with the two ends of the box, having been made water-tight by means of collars of oiled leather, the box was filled with cold water, (viz. at the temperature of 60°) and the machine was put in motion.

The refult of this beautiful experiment was very ftriking, and the pleafure it afforded me amply repaid me for all the trouble I had had in contriving and managing the complicated machinery ufed in making it.

The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a fhort time, when I perceived, by putting my hand into the water and touching the outfide of the cylinder, that heat had been generated; and it was not long before the water which furrounded the cylinder began to be fenfibly warm.

At the end of one hour I found, by plunging a thermometer into the water in the box, (the quantity of which fluid amounted to 18.77 lb. avoirdupois, or 2¼ wine gallons,) that its temperature had been raifed no lefs than 47 degrees, being now 107° of Fahrenheit's fcale.

When

When 30 minutes more had elapſed, or 1 hour and 30 minutes after the machinery had been put in motion, the heat of the water in the box was found to be raiſed to 178°.

At 2 hours 20 minutes, it was at 200°; and at 2 hours 30 minutes it ACTUALLY BOILED.

It would be difficult to deſcribe the ſurpriſe and aſtoniſhment expreſſed in the countenances of the byſtanders, on ſeeing ſo large a quantity of cold water heated, and actually made to boil, without any fire.

Though there was, in fact, nothing that could juſtly be conſidered as ſurpriſing in this event, yet I acknowledge, fairly, that it afforded me a degree of childiſh pleaſure, which, were I ambitious of the reputation of a grave *philoſopher*, I ought moſt certainly rather to hide than diſcover.

The quantity of heat excited and accumulated in this experiment was very conſiderable; for, not only the water in the box, but alſo the box itſelf, (which weighed 15¼lb.) and the hollow metallic cylinder, and that part of the iron bar which, being ſituated within the cavity of the box, was immerſed in the water, were heated through 150 degrees of Fahrenheit's ſcale; viz. from 60 degrees (which was the temperature of the water and of the machinery at the beginning of the experiment) to 210 degrees, the heat of boiling water at Munich.

The total quantity of heat generated may be eſtimated with ſome conſiderable degree of preciſion as follows:

Of the heat excited there appears to have been actually accumulated,	Quantity of ice-cold water which, with the given quantity of heat, might have been heated to 180°, or made to boil. In Avoirdupois weight. lb.
In the water contained in the wooden box, 18¾lb. avoirdupois, heated 150 degrees; namely, from 60° to 210° F. - -	15.2
In 113.13lb. of gun metal (the hollow cylinder) heated 150 degrees; and, as the capacity for heat of this metal is to that of water as 0.1100 to 1.0000, this quantity of heat would have heated 12½lb. of water the ſame number of degrees. - -	10.37
In 36.75 cubic inches of iron, (being that part of the iron bar to which the borer was fixed which entered the box,) heated 150 degrees; which may be reckoned equal in capacity for heat to 1.21lb. of water. - - - - -	1.01
N. B. No eſtimate is here made of the heat accumulated in the wooden box, nor of that diſperſed during the experiment.	
Total quantity of ice-cold water which, with the heat actually generated by friction, and accumulated in 2 hours 30 minutes, might have been heated 180 degrees, or made to boil. - -	26.58

From the knowledge of the *quantity* of heat actually produced in the foregoing experiment, and of the *time* in which it was generated, we are enabled to aſcertain *the velocity of its production*, and to determine how large a fire muſt have been made, or how much fuel

muſt have been conſumed, in order that, in burning equably, it ſhould have produced by combuſtion the ſame quantity of heat in the ſame time.

In one of Dr. Crawford's experiments, (ſee his Treatiſe on Heat, p. 321) 37lb. 7oz. troy = 181920 grains of water were heated $2\frac{1}{10}$ degrees of Fahrenheit's thermometer, with the heat generated in the combuſtion of 26 grains of wax. This gives 382032 grains of water heated 1 degree with 26 grains of wax; or $14693\frac{14}{26}$ grains of water heated 1 degree, or $\frac{14693}{180} = 81.631$ grains, heated 180 degrees with the heat generated in the combuſtion of 1 grain of wax.

The quantity of ice-cold water, which might have been heated 180 degrees with the heat generated by friction in the before-mentioned experiment, was found to be 26.58lb. = 188060 grains; and, as 81.631 grains of ice-cold water require the heat generated in the combuſtion of 1 grain of wax to heat it 180 degrees, the former quantity of ice-cold water, namely, 188060 grains, would require the combuſtion of no leſs than 2303.8 grains ($= 4\frac{8}{10}$oz. troy) of wax to heat it 180 degrees.

As the experiment (No. 3.) in which the given quantity of heat was generated by friction laſted 2 hours 30 minutes = 150 minutes; it is neceſſary, for the purpoſe of aſcertaining how many wax candles of any given ſize muſt burn together, in order that, in the combuſtion of them, the given quantity of heat may be generated in the given time, and conſequently *with the ſame celerity* as that with which the heat was generated by friction in the experiment, that the ſize of the candles ſhould be determined, and the quantity of wax conſumed in a given time by each candle in burning equably ſhould be known.

Now, I found by an experiment made on purpoſe to finiſh theſe computations, that when a good wax candle of a moderate ſize, $\frac{3}{4}$ of an inch in diameter, burns with a clear flame, juſt 49 grains of wax are conſumed in 6 minutes. Hence it appears, that 245 grains of wax would be conſumed in 30 minutes; and that to burn the quantity of wax (= 2303.8 grains) neceſſary to produce the quantity of heat actually obtained by friction, in the experiment in queſtion, and in the given time (150 minutes), nine candles burning at once would not be ſufficient; for 9 multiplied into 245 (the number of grains conſumed by each candle in 150 minutes) amounts to no more than 2205 grains; whereas the quantity of wax neceſſary to be burned, in order to procure the given quantity of heat, was found to be 2303.8 grains.

From the reſult of theſe computations it appears, that the quantity of heat produced equably, or in a continual ſtream (if I may uſe that expreſſion), by the friction of the blunt ſteel borer againſt the bottom of the hollow metallic cylinder, in the experiment under conſideration, was greater than that produced equably in the combuſtion of 9 wax candles, each $\frac{3}{4}$ of an inch in diameter, all burning together, or at the ſame time, with clear bright flames.

As the machinery uſed in this experiment could eaſily be carried round by the force of one horſe (though, to render the work lighter, two horſes were actually employed in doing it), theſe computations ſhow farther how large a quantity of heat might be produced by proper mechanical contrivance, merely by the ſtrength of a horſe, without either fire, light, combuſtion, or chemical decompoſition; and, in a caſe of neceſſity, the heat thus produced might be uſed in cooking victuals.

But no circumſtances can be imagined in which this method of procuring heat would not be diſadvantageous; for more heat might be obtained by uſing the fodder neceſſary for the ſupport of a horſe as fuel.

As ſoon as the laſt-mentioned experiment (No. 3.) was finiſhed, the water in the wooden box was let off, and the box removed; and the borer being taken out of the cylinder, the ſcaly metallic powder, which had been produced by the friction of the borer againſt the bottom of the cylinder, was collected, and, being carefully weighed, was found to weigh 4145 grains, or about $8\frac{2}{3}$ oz. troy.

As this quantity was produced in $2\frac{1}{2}$ hours, this gives 830 grains for the quantity produced *in half an hour.*

In the firſt experiment, which laſted only half an hour, the quantity produced was 837 grains.

In the experiment No. 1, the quantity of heat generated in *half an hour* was found to be equal to that which would be required to heat 5lb. avoirdupois of ice-cold water 180 degrees, or cauſe it to boil.

According to the reſult of the experiment No. 3, the heat generated *in half an hour* would have cauſed 5.31lb. of ice-cold water to boil. But in this laſt-mentioned experiment, the heat generated being more effectually confined, leſs of it was loſt; which accounts for the difference of the reſults of the two experiments.

It remains for me to give an account of one more experiment which was made with this apparatus. I found, by the experiment No. 1, how much heat was generated when the air had free acceſs to the metallic ſurfaces which were rubbed together. By the experiment No. 2, I found that the quantities of heat generated were not ſenſibly diminiſhed when the free acceſs of the air was prevented; and, by the reſult of No. 3, it appeared that the generation of heat was not prevented or retarded by keeping the apparatus immerſed in water. But as, in this laſt-mentioned experiment, the water, though it ſurrounded the hollow metallic cylinder on every ſide, externally, was not ſuffered to enter the cavity of the bore (being prevented by the piſton), and conſequently did not come into contact with the metallic ſurfaces where the heat was generated; to ſee what effects would be produced by giving the water free acceſs to theſe ſurfaces, I now made the

Experiment No. 4.

THE piſton which cloſed the end of the bore of the cylinder being removed, the blunt borer and the cylinder were once more put together; and the box being fixed in its place, and filled with water, the machinery was again put in motion.

There was nothing in the reſult of this experiment that renders it neceſſary for me to be very particular in my account of it. Heat was generated as in the former experiments, and to all appearance quite as rapidly; and I have no doubt but the water in the box would have been brought to boil, had the experiment been continued as long as the laſt. The only circumſtance that ſurpriſed me was, to find how little difference was occaſioned in the noiſe made by the borer in rubbing againſt the bottom of the bore of the cylinder, by filling the bore with water. This noiſe, which was very grating to the ear, and ſometimes almoſt inſupportable, was, as nearly as I could judge of it, quite as loud and as diſ-

agreeable when the surfaces rubbed together were wet with water, as when they were in contact with air.

By meditating on the results of all these experiments, we are naturally brought to that great question which has so often been the subject of speculation among philosophers, namely, What is heat?—Is there any such thing as an *igneous fluid?*—Is there any thing that can with propriety be called caloric?

We have seen that a very considerable quantity of heat may be excited in the friction of two metallic surfaces, and given off in a constant stream or flux *in all directions*, without interruption or intermission, and without any signs of diminution or exhaustion.

From whence came the heat which was continually given off in this manner in the foregoing experiments? Was it furnished by the small particles of metal detached from the larger solid masses on their being rubbed together? This, as we have already seen, could not possibly have been the case.

Was it furnished by the air? This could not have been the case; for in three of these experiments, the machinery being kept immersed in water, the access of the air of the atmosphere was completely prevented.

Was it furnished by the water which surrounded the machinery? That this could not have been the case is evident; *first*, because this water was continually *receiving heat* from the machinery, and could not at the same time be *giving to* and *receiving heat from* the same body; and, *secondly*, because there was no chemical decomposition of any part of this water. Had any such decomposition taken place (which indeed could not reasonably have been expected), one of its compound elastic fluids (most probably inflammable air) must at the same time have been set at liberty, and, in making its escape into the atmosphere, would have been detected; but though I frequently examined the water to see if any air bubbles rose up through it, and had even made preparations for catching them in order to examine them if any should appear, I could perceive none; nor was there any sign of decomposition of any kind whatever, or other chemical process going on in the water.

Is it possible the heat could have been supplied by means of the iron bar to the end of which the blunt steel borer was fixed? or by the small neck of gun-metal by which the hollow cylinder was united to the cannon? These suppositions appear more improbable even than either of those before mentioned; for heat was continually going off or *out of the machinery*, by both these last passages, during the whole time the experiment lasted.

And, in reasoning on this subject, we must not forget to consider that most remarkable circumstance, that the source of the heat generated by friction in these experiments appeared evidently to be inexhaustible.

It is hardly necessary to add, that any thing which any *insulated* body or system of bodies can continue to furnish *without limitation*, cannot possibly be a *material substance*; and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of any thing capable of being excited and communicated in the manner the heat was excited and communicated in these experiments, except it be motion.

I am very far from pretending to know how, or by what means or mechanical contrivance, that particular kind of motion in bodies which has been supposed to constitute heat

is excited, continued, and propagated; and I ſhall not preſume to trouble the Society with mere conjectures, particularly on a ſubject which, during ſo many thouſand years, the moſt enlightened philoſophers have endeavoured but in vain to comprehend.

But although the mechaniſm of heat ſhould in fact be one of thoſe myſteries of nature which are beyond the reach of human intelligence, this ought by no means to diſcourage us, or even leſſen our ardour, in our attempts to inveſtigate the laws of its operations. How far can we advance in any of the paths which ſcience has opened to us, before we find ourſelves enveloped in thoſe thick miſts which on every ſide bound the horizon of the human intellect? But how ample and how intereſting is the field that is given us to explore!

Nobody, ſurely, in his ſober ſenſes has ever pretended to underſtand the mechaniſm of gravitation; and yet what ſublime diſcoveries was our immortal NEWTON enabled to make, merely by the inveſtigation of the laws of its action!

The effects produced in the world by the agency of heat are probably *juſt as extenſive*, and quite as important, as thoſe which are owing to the tendency of the particles of matter towards each other; and there is no doubt but its operations are in all caſes determined by laws equally immutable.

Before I finiſh this paper I would beg leave to obſerve, that although, in treating the ſubject I have endeavoured to inveſtigate, I have made no mention of the names of thoſe who have gone over the ſame ground before me, nor of the ſucceſs of their labours; this omiſſion has not been owing to any want of reſpect for my predeceſſors, but was merely to avoid prolixity, and to be more at liberty to purſue without interruption the natural train of my own ideas.

Deſcription of the Figures. Plate V.

FIG. 1. ſhows the cannon uſed in the foregoing experiments, in the ſtate it was when it came from the foundry.

Fig. 2. ſhows the machinery uſed in the experiments No. 1. and No. 2. The cannon is ſeen fixed in the machine uſed for boring cannon. W is a ſtrong iron bar (which, to ſave room in the drawing, is repreſented as broken off); which bar being united with machinery (not expreſſed in the figure) that is carried round by horſes, cauſes the cannon to turn round its axis.

m is a ſtrong iron bar, to the end of which the blunt borer is fixed, which, by being forced againſt the bottom of the bore of the ſhort hollow cylinder that remains connected by a ſmall cylindrical neck to the end of the cannon, is uſed in generating heat by friction.

Fig. 3. ſhows on an enlarged ſcale the ſame hollow cylinder that is repreſented on a ſmaller ſcale in the foregoing figure. It is here ſeen connected with the wooden box (*g*, *h*, *i*, *k*) uſed in the experiments No. 3. and No. 4. when this hollow cylinder was immerſed in water.

p, which is marked by dotted lines, is the piſton which cloſed the end of the bore of the cylinder.

n is the blunt borer ſeen ſideways.

d, *e*, is the ſmall hole by which the thermometer was introduced, that was uſed for aſcertaining the heat of the cylinder. To ſave room in the drawing, the cannon is repreſented broken off near its muzzle; and the iron bar, to which the blunt borer is fixed, is repreſented broken off at *m*.

Fig.

Fig. 4. is a perspective view of the wooden box, a section of which is seen in the foregoing figure. (See *g*, *h*, *i*, *k*, fig. 3.)

Fig. 5. and 6. represent the blunt borer n joined to the iron bar *m* to which it was fastened.

Fig. 7. and 8. represent the same borer with its iron bar, together with the piston, which, in the experiments No. 2. and No. 3. was used to close the mouth of the hollow cylinder.

IV.

An Attempt to discover the Genuineness and Purity of Drugs and Medical Preparations. *By* FREDERICK ACCUM.

To Mr. NICHOLSON.

SIR,

OF all the possible frauds hitherto practised by mercenary traders, there is none more intimately connected with the welfare of mankind than the art of making or counterfeiting drugs and adulterating genuine medicines.

This unprincipled art is brought to such perfection, that spurious articles are every where to be found in the market, made up with such dexterity, that not only the mere merchant and drug broker, but even the man of skill, is sometimes deceived.

The influence of this practice has indeed been observed by medical men of science, and the rulers of this art have thought it their duty to appoint a court of examiners to investigate the goodness of drugs and medicines in the different chemists' and apothecaries' shops in this metropolis.

How far this great work is accomplished, or to what extent the whole undertaking does either honour and credit to the sagacity of these learned, liberal, and upright examiners, or affords justice to the public, I shall leave to the determination of every individual who has once only been honoured with their visit.

But as moral duty should stimulate every individual to offer all the assistance in his power to suppress or check such detestable practices, I have ventured to recommend that method which, if judiciously executed, will contribute in no small degree to the public good in this respect. If these sketches should tend in any measure to put the unwary on their guard, I shall think myself amply rewarded, and shall not fail to extend them from time to time, and lay them before the public with some confidence.

No. 17, *Hay-market,*
May 1st, 1798.

FRED. ACCUM.

EXAMINATION OF SALINE SUBSTANCES.

Sulphuric Acid.

THE vitriolic acid kept in our shops under the name of oil of vitriol, usually contains not only lead in solution, but likewise iron, copper, and sulphate of potash.

In order to discover these admixtures, a little of the acid must be diluted with distilled water and saturated with pure vegetable alkali, as by this means the copper, iron, and

5 lead

lead become precipitated in the form of a more or leſs dark-coloured powder, which is to be ſeparated by the filter, and waſhed with diſtilled water.

To analyſe this precipitate, it is firſt treated with pure ammoniac, which ſoon acquires a blue colour if copper be preſent.——The blue ſupernatant fluid is to be ſeparated, the remainder waſhed in diſtilled water, and then diſſolved in pure muriatic acid.

In order to aſcertain the preſence of lead, the foregoing ſolution is mixed with an equal quantity of water impregnated with ſulphurated hydrogen gas; and if a dark brown or black precipitate enſues, it is a ſure ſign that lead is preſent.

Iron that may happen to be in ſolution is exhibited by the admixture of a few drops of pure pruſſiate of potaſh or tincture of gall-nuts, which in the firſt caſe will produce a blue, and in the ſecond a black precipitate.

Sulphate of potaſh, or the vegetable alkali, united to ſulphuric acid is more difficult to diſcover, as for this purpoſe a certain quantity of the acid is to be perfectly ſaturated with carbonate of potaſh, or carbonate of ammoniac; to this ſolution a little tartareous acid is to be added, which by its ſuperior affinity will decompoſe the vitriolated tartar, and indicate the vegetable alkali under the form of tartarite of potaſh ſeparated from the fluid. If the quantity in the acid be not very ſmall, as is ſeldom the caſe, it may alſo be diſcovered by the admixture of ſtrong ardent ſpirit; for by this means a quantity of ſulphate of potaſh will be ſeparated in the form of a white powdery precipitate.

The ſpecific gravity of ſulphuric acid, according to our pharmacopœia, ſhould be to that of diſtilled water as 185 to 100.

It is void of ſmell—perfectly colourleſs—and boils at 546° of Fahrenheit's thermometer.

Equal parts of vitriolic acid and water, haſtily mixed together, ſhould produce a heat nearly equal to that of boiling water.

Nitrous Acid.*

THE nitrous acid of commerce (or double aqua fortis as it is commonly called) always contains either ſulphuric or muriatic acid, and often both.

The vitriolic acid may be diſcovered by the addition of a few drops of a ſolution of nitrate of barytes, or acetite of lead, which form inſoluble compounds and fall down. The muriatic acid is manifeſted by the addition of a ſolution of nitrate of ſilver. In this caſe muriate of ſilver is formed and precipitated.

Its ſpecific gravity ſhould be to that of diſtilled water, as 155 to 100.

If it is of an orange yellow colour, and emits a vaſt quantity of elaſtic fluid, known by the name of nitrous gas, which forms yellow fumes in our atmoſphere. Mixed with water it develops a conſiderable degree of heat †.

* The inſtruction for preparing nitrous acid according to our pharmacopœia is erroneous, as the proportion of ſulphuric acid preſcribed is much too ſmall: it conſequently leaves a conſiderable quantity of nitrate of potaſh behind undecompoſed. A.

† Pure nitrous acid is deprived of a portion of nitrous gas by the application of heat. It is then colourleſs, and conſtitutes nitric acid. Expoſure of nitric acid to light expels oxygen, and leaves the acid with that redundancy of azote which conſtitutes the nitrous or yellow acid. Nitric acid muſt therefore be kept in the dark, or elſe in bottles quite full. N.

Muriatic

Muriatic Acid *.

COMMONLY called ſpirit of ſalt, always contains iron, and frequently ſulphuric acid and copper.

The ſulphuric acid is detected either by the addition of barytes, or acetite of lead, and the reſults are the ſame as in the foregoing experiments.

Copper is inveſtigated by ſuperſaturating this acid with pure ammoniac, which in this caſe will immediately be tinged of a perceptible blue.

Iron is manifeſted by previouſly uniting this acid with carbonate of potaſh and treating it then with tincture of gall-nuts, or pruſſiate of potaſh. The firſt will produce a black, and the latter a blue precipitate.

The ſpecific gravity of muriatic acid ſhould be, 1,70 to 1,000.

Pure muriati c acid is totally deſtitute of colour.

Acetous Acid.

THE concentrated acetous acid commonly met with contains ſulphuric acid, with which it is either fraudulently adulterated, or with which it is impregnated by a careleſs and ſlovenly management during the proceſs for obtaining it.

We often find this acid contaminated by tartareous acid, and not unfrequently by copper or lead. The preſence of ſulphuric acid is here alſo diſcovered by the addition of barytes, or acetite of lead, in the manner already mentioned.

Tartareous acid is detected by ſaturating the acetous acid with vegetable alkali, in conſequence of which a tartarite of potaſh becomes ſeparated in the form of a white powder.

Copper is diſcovered by ſaturating the acid with carbonate of ammoniac, and lead becomes manifeſted by mixing it with water impregnated with ſulphurated hydrogenous gas. In the firſt caſe a blue, and in the ſecond a black precipitate will be formed.

The ſpecific gravity of the moſt concentrated acetous acid ſhould be to that of water as 1,050 to 1,000. It is colourleſs, and of a penetrating ſmell.

Diſtilled Vinegar.

VINEGAR, if not diſtilled in glaſs veſſels, but in a ſtill with a pewter head, always contains lead in ſolution.

To diſcover this, equal quantities of vinegar, and water impregnated with ſulphurated hydrogenous gas, are mixed together, which mixture, if the vinegar is free from lead, will remain unaltered; whereas, on the contrary, the ſmalleſt quantity of this metal will produce a black precipitate.

We frequently meet with diſtilled vinegar adulterated with ſulphureous acid, the preſence of which is detected by means of nitrate of barytes, or acetite of lead, as mentioned before.

* The proportion of ſulphuric acid preſcribed by our royal college of phyſicians for obtaining this acid is much too great, as 18 or 20 ounces are quite ſufficient for a total decompoſition of the quantity of muriate of potaſh ordered.

The

The beſt diſtilled vinegar has a pleaſant taſte and fragrant ſmell, is perfectly colourleſs, and twelve parts of it require one of dry vegetable alkali to neutraliſe it.

Boracic Acid.

ON account of the high price which this acid bears, we often meet with it intentionally adulterated with Venetian talc, aſbeſt, &c.

Genuine boracic acid is ſoluble in five times its quantity of boiling ardent ſpirit, and the ſolution when ſet on fire burns with a green flame. In water it diſſolves tardily; when ſubmitted to the blow-pipe it fluxes to a perfect tranſparent glaſs if genuine; it vitrifies earth and ſtones. If it be added in ſmall quantities to tartarite of potaſh, it tranſforms it into a very ſoluble ſalt. The beſt boracic acid is exhibited in ſmall hexangular ſcaly cryſtals, of a ſhining ſilvery white colour. It diſcovers only a ſlight acid taſte, and does not deliqueſce in the air.

Its ſpecific gravity is 1,480.

Tartareous Acid.

TARTAREOUS acid may very eaſily be contaminated with ſulphuric acid, either fraudulently or in conſequence of a faulty preparation.

The admixture of ſulphuric acid is ſoon diſcovered, if a ſmall quantity of the tartareous acid be diſſolved in diſtilled water, and a few drops of a ſolution of acetite of lead are added to it: by this means a white precipitate is afforded, which, by the addition of a few drops of pure nitric acid, will be entirely rediſſolved if no ſulphuric acid is preſent. The preſence of this acid is ſtill more readily manifeſted by adding to the tartareous acid diſſolved in diſtilled water a few drops of a ſolution of nitrate of barytes, in conſequence of which ſulphate of barytes will immediately be generated.

Pure cryſtallized tartareous acid does not change by expoſure to air; it is very ſoluble in water, and poſſeſſes a grateful acid taſte.

Acid of Amber.

WE find this acid wonderfully adulterated ſometimes with ſulphuric acid and all its combinations, ſometimes with tartareous acid, and now and then with muriate of ammoniac.

The ſulphuric acid is diſcovered by means of nitrate of barytes, or acetite of lead.

The tartareous acid is diſcovered by means of vegetable alkali; for, if this acid be preſent, a quantity of tartarite of potaſh will be formed.

Muriate of ammoniac is diſcovered with reſpect to one of its component parts (the muriatic acid), by the ſolution of nitrate of ſilver; and with reſpect to the other (the ammoniac) by adding to the aqueous ſolution of this ſalt a little vegetable alkali, and, after heating the mixture, holding over it a ſtopper moiſtened with acetous acid; white fumes will be formed, which indicate that ammoniac is preſent.

Pure acid of amber is a cryſtalline white ſalt, of an acid taſte, ſoluble in twenty-four times its weight of cold water, but in eight when boiling hot, and is volatilized on an ignited iron, leaving neither aſhes nor any other reſidue behind.

Acid of Benzoin,

WHICH is commonly known by the name of flowers of benjamin, is not liable to be easily adulterated.

The best flowers of benjamin are brilliant white, possess a peculiarly grateful smell, are totally soluble in boiling water and ardent spirit, and leave no residue or ashes when laid upon a red-hot iron, or on ignited coals.

V.

A Model proposed for the Construction of a Satellitian, or Instrument for explaining the Phenomena of Jupiter and his Satellites, with an Account of its Use. By the Rev. W. PEARSON, of Lincoln.*

SINCE the doctrine of brightness has been particularly attended to by Mr. Herschell in his observations of the heavenly bodies, this celebrated astronomer has been able, by means of the excellence of his telescopes, to announce to the world, what his predecessors could only conjecture, not only that the satellites of Jupiter have each a rotatory motion on their axes, but also the exact time of each rotation. In Part II. of the Philosophical Transactions of the Royal Society of the last year [1797], is an article in which Mr. Herschell has proved, I think, in a satisfactory manner, that each of these four satellites has, like our moon, just one rotation in every periodical revolution round its primary, viz. The first or nearest, in 1 day 8 hours and $26\frac{6}{10}$ minutes; the second, in 3 days 13 [by a typographical error made 18] hours and $17\frac{9}{10}$ minutes; the third, in 7 days 3 hours and $59\frac{3}{10}$ minutes; and the fourth, in 16 days 18 hours and $5\frac{1}{10}$ minutes.——This discovery of a rotation in each revolution renders the analogy so striking between the Jovian, as a detached part of the solar system, and that of our own earth and moon, that we can hardly any longer doubt whether or not Jupiter be inhabited. Such a consideration, in my opinion, renders the Jovian portion of the solar system a subject which merits more particular attention than is usually allotted to it in lectures upon astronomy; and on this account I have contrived the model of an instrument of a simple construction for the purpose of explaining the different phenomena thereof, which, for want of a more appropriate name, I shall take the liberty of calling a satellitian. Other instruments to answer the same purpose may have been constructed and used; but as I have no knowledge of any, except that Mr. Rowley's grand orrery is said to represent Jupiter's satellites moving by wheel-work†, I feel myself warranted in presenting to the public a plan and description of the constituent parts of this detached instrument, and of the method of using it, in hopes that it may prove not unacceptable to the cultivators of science, or at least that it may suggest to some more ingenious artist the means of making a more eligible one.

* Communicated by the author.

† A machine for this purpose is also described in Harris's Lexicon Technicum. N.

In

In figure 1 of plate VI. is exhibited a lateral view of the wheels of this instrument, which are calculated for producing the mean motions of the four satellites, and contained in the brass box ABCD, $8\frac{1}{10}$ inches long within, 4 broad, and $1\frac{1}{10}$ deep. Figure 2, EF, represents the cover of this box, on which the different faces for the hour, week, and month-hands are graduated and silvered: the small concentric wheels marked 8, 18, 27, and 39 in fig. 1, are all fixed on the same revolving axle, placed on the point H, which carries the hour-hand on its upper end once round in 24 hours; but the larger corresponding wheels, 134, 129, 96, and 69, are fixed on the ends of as many separate tubes or hollow axles of brass, which just turn easily round within one another, and are supported by a shoulder in the perpendicular fixed stem that holds Jupiter over the point S; the innermost being the longest, and the next to the innermost the next in length, as in a common planetarium: these tubes have each a broad ring of a proper diameter to slide a little way upon their upper ends, to which are riveted the crooked arms which support the satellites; and the rings are turned round the tubes in the adjustment of the satellites to their relative situations: the tubes also have shoulders each for the support of its next superior wheel, and are kept in their places by the lower edges of the rings.

All the parts of the satellitian are laid down in the plate in their proper proportions and full size, according to the following table, from which an artist of but little ingenuity may easily construct it, provided he is furnished with, or has access to, a cutting engine for making the wheels, the plate of which is properly divided for the required numbers. An easy method for dividing a plate is laid down in Mr. Ferguson's "Select Exercises," in substance thus: As all the teeth in any wheel are to 360°, so are any odd number of teeth, to the number of degrees to be marked out, and divided into that odd number; after which the remaining even number may be very easily divided by continued halves.

Table of Dimensions *.

Wheels.	Diam. in Inches from the Pitch Line.	Distance of the Satellites from Jupiter in Diameters of him.
134	2,8	1. Satellite $2\frac{5}{6}$ diameters.
129	2,6	2. do. — $4\frac{1}{2}$ do.
96	2,3	3. do. — $7\frac{1}{6}$ do.
69	1,9	4. do. — $12\frac{2}{3}$ do.
8	0,2	
18	0,4	From S to H $1\frac{1}{2}$ inches.
27	0,7	From H to W 0,68 do.
39	1,1	From W to P 2,125 do.
56	1,2	
7	0,35	
73	3,9	

* The wheels in the table are proportioned thus:—As the sum of the teeth in any two corresponding wheels is to the distance between their centres, or sum of their semidiameters, so is the number of teeth in each, taken separately, to its semidiameter: an allowance being afterwards made for the driving wheel being somewhat larger in this proportion than the driven one, to make them work more easily.——W. P.

Though

Though the dimenſions of the wheelwork here given are convenient for the conſtruction of the ſatellitian, yet the box may be in the form of a ſquare, an octagon, or a circle; though a parallelogram ſeems to be the moſt portable; but of whatever ſhape, there muſt be a contrivance for cramping or otherwiſe fixing it to a table when uſed.—The ſize of the ball, which repreſents Jupiter in the plate, is proportioned to the length of the arms which carry the ſatellites, and ought not to be larger; for an increaſed length of the arms would not admit the ſcreen to approach near enough to prevent the divergence of the ſhadows.—The balls which repreſent the ſatellites ſhould, however, be as diminutive as poſſible, ſo that they give a diſtinct ſhadow by candlelight. According to Mr. Herſchell's obſervations, the third is the largeſt, the firſt and fourth nearly equal, and the ſecond the ſmalleſt: their colours are, in his own words, as follows: the firſt is "white, more ſo ſometimes than others;" the ſecond "white, blueiſh, and aſh-coloured;" the third "white, differently in different ſituations;" and the fourth "duſky, dingey inclining to orange, reddiſh, and ruddy at different times."

A motion is communicated to the machinery by means of a ſingle endleſs ſcrew, on an horizontal axle, lying acroſs the box at the ſmall black circle near 73, and working with the teeth of this wheel: a handle is put upon the projecting end of this axle, which cannot appear in the plate, at ſuch a diſtance from the ſide of the box, as may prevent the hand of the perſon who turns it from touching any of the ſmall balls, or intercepting the view of a ſmall company of ſpectators.

As appendages to the ſatellitian, a ſmall ſliding ſcreen of thin paper and an appropriate candleſtick are neceſſary.—The mode of adjuſting the ſcreen is optional, and therefore requires no deſcription.—The baſe of the candleſtick, of which a ſketch is given in miniature at fig. 3, conſiſts of a circular plate of braſs 4 inches diameter, divided into the 12 ſigns, and graduated in concentric circles near the extremity: in the centre of this ſtands a ſhort perpendicular ſtem of a cylindrical form, which ſcrews into a ſolid piece ſoldered on the ſurface of the plate, and has a ſocket proper for containing a candle or lamp at the upper end: on the low end of this ſtem turns a broad ring, into which an arm is riveted 3 inches long, which has a ſimilar ſocket on its upper end, and oppoſite thereto a hand alſo riveted, which points to the graduated ecliptic oppoſite to the part on which the arm reſts at all times.

In order to eſtimate the accuracy of the mean motions of the little balls in the ſatellitian, we muſt conſider each pair of correſponding wheels as an improper fraction of a day; thus $\frac{69}{39}$ of 24 hours is equal to 1 d. 18 h. 27 min. 41 ſec.; $\frac{96}{27}$ is equal to 3 d. 13 h. 20 min.; $\frac{129}{18}$ is equal to 7 d. 4 h. 0 min.; and $\frac{134}{8}$ equal to 16 d. 18 h. 0 min.; whence it appears that the error in one day's mean motion of the firſt ſatellite is not quite $+ 37''$; of the ſecond ſomewhat more than $+ 37''$; of the third not quite $+ 3''\frac{1}{2}$; and of the fourth about $- 18''$.——The firſt and ſecond of theſe errors will amount to one hour's motion of each ſatellite in about $97\frac{1}{2}$ days' uſe with one rectification; but the third and fourth will not be perceptible by the eye in ſeveral years, by reaſon of the ſlowneſs of the ſatellites' motions, as well as ſmallneſs of their daily errors. With regard to the calculation of the other wheels, the fraction $\frac{56}{8}$ is equal to 7 days for the week-hand; and the compound one $\frac{56}{8} \times \frac{73}{7}$ is equal to 73; but the ſpiral face on the cover has five lines divided into

365 equal

365 equal parts, therefore five revolutions of the month-hand, viz. 5 × 73, will complete the year. The 365 days in the spiral are divided into calendar months, the initials of each of which stand immediately over the beginning of the first day of each; so that when the instrument is rectified for the month, day, and hour at any particular time, the different hands will preserve their respective situations to the end of the year, and so on from year to year, provided the month-hand be put back one day in the spiral at the latter end of February in every leap year. This putting back, however, will not in the least affect the motions of the satellites; for the hands, being put upon the circular ends of the axles, will turn in adjustment without giving motion to the wheels.

As the error in the first and second satellites will amount in one year to about three hours and forty-four minutes motion of each, they must also after that space be put to their right places again, which is very easily done thus: Turn the handle in a retrograde direction till all the satellites have come back 4 h. 44 min. then hold the first and second in that particular position while the handle is again turned to its original situation, and then let them be at liberty.—If this is done at a quarter of a year after rectification, the error will be somewhat less than an hour, and it may render a new rectification by the nautical almanac, as will be described hereafter, needless.

If at any time the letter pointed to in the week circle, and the corresponding day of the month in the spiral, in the first, second, third, fourth, or fifth line, as the case may be, be both attentively observed, it will appear by inspection for what particular time the satellitian stands rectified in a given year after it has been out of use; for, as the month-hand lies over the five lines, that particular line is the proper one, the numerical point of which answers to the initial of the day to which the week-hand points: for example, in the plate the rectification of the hands is for noon of Wednesday June 14, 1797. Without a regard to such an inspection, a new rectification would become necessary every time the instrument was to be used, and in some cases probably when the means of rectification might not be immediately attainable.

If the orbits of the satellites were not necessarily too large in the satellitian to be proportional to those of the earth and Jupiter, when the latter is placed at such a distance from them as that a candle in its place may produce a distinct shadow of each, $5\frac{2}{10}$ times the distance between the two sockets, viz. $15\frac{6}{10}$ inches, would be the proper distance of the central candle from Jupiter, in which position the angle at Jupiter in the satellitian subtended by the distance between the sockets, viz. 3 inches, would be equal to the parallax of the earth's annual orbit seen from Jupiter; but as the exact proportions of all their orbits cannot be preserved, a mechanical adjustment becomes requisite to preserve the due arrangement of the shadows upon the screen.

The rectification ought always to be very minutely attended to, and is thus effected: Fix the satellitian firmly on a table, and, setting the centre of the candlestick just *four feet* from Jupiter, place a steady burning candle, or rather a lamp, in the central socket, upon the same level with Jupiter in the instrument, and a similar one in the socket supported by the arm at the same height; by this means two shadows of Jupiter will fall upon the screen when placed behind the satellitian: slide the screen as near Jupiter as a revolution of the longest arm will suffer it to approach, which will be upwards of $2\frac{1}{4}$ inches; and if the

the central candle and Jupiter form a right angle at the other candle, the two ſhadows will cover one another juſt ſo much as is repreſented in fig. 4. or fig. 6. ; but if this is not exactly the caſe, the candleſtick muſt be brought nearer, or removed farther, till this appearance is produced ; and then the ſcreen muſt be ſecured in its place, and the candleſtick be ſuffered to remain. In the next place, with a pair of bow-compaſſes deſcribe a circle coincident with that ſhadow which is occaſioned by the central candle ; and within that, three other concentric circles, the largeſt of the three to touch the edge of the other ſhadow when the arm remains unmoved ; the next to touch the ſame when the arm is 46° from conjunction with Jupiter and the central candle ; and the ſmalleſt to touch the ſame when the arm is 24° from a ſimilar ſituation.

Here then the central candle will repreſent the ſun ; the outermoſt circle in fig. 4. or 6. the diametrical ſection of the ſun's ſhadow at the orbit of the firſt ſatellite ; the ſecond circle is ſuppoſed to repreſent the appearance of the ſame at the orbit of the ſecond ; the third, the appearance at that of the third ; and the ſmall one, the appearance at that of the fourth, as viewed in perſpective from the earth ; whilſt the candle carried by the arm will be the repreſentative of the earth. Remove now the central candle, and Jupiter's remaining ſhadow will be his ſituation as viewed from the earth, coinciding with his real ſhadow, repreſented by the concentric circles, as far as the orbit of the ſecond ſatellite, and then diſuniting.

Hitherto the ſatellites themſelves have been diſregarded, and muſt next be placed in their proper ſituations by the help of a * nautical almanac, thus : Look for the day, for which the ſatellitian is to be rectified, among the configurations given in the laſt page of every month, and the relative *apparent* ſituations of all the ſatellites, at the hour ſpecified at the top, will be found to the right and left of the central cypher which repreſents Jupiter. When the numerical figure ſtands between the repreſentative point of any ſatellite and Jupiter, the ſatellite is approaching him ; but when the point is put the nearer, it is receding from him : alſo, when a ſatellite is approaching on the right hand of Jupiter, or receding on the left, it is in its ſuperior ſemicircle, and is placed above the centre of the cypher ; but when receding on the right, or approaching on the left, it is in its inferior one, and ſtands higher than the centre. The relative ſituations and direction of motion of each ſatellite being obſerved, put them all to their places as near as the eye can gueſs by the ſhadows compared to the points in the almanac ; then, having previouſly marked the greateſt elongation of the fourth with little points on the ſcreen, by means of a ſector or diagram of ſimilar triangles, make the diſtance of each ſhadow, from Jupiter's centre on the ſcreen, bear the ſame proportion to the diſtance of each point from the centre of the cypher in the almanac, that the greateſt elongation of the fourth on the former bears to the greateſt elongation of the ſame in the latter: this will be attended with no difficulty.— The rectification for the *true* places of *mean motion* will however be the more accurate the

* The rectification may be made moſt accurately by calculating the time of a conjunction of each ſatellite, *mean* or *apparent*, as may be required, by the help of Wargentin's Tables, in any given month : but every reader cannot be ſuppoſed to have ſuch Tables ; and therefore the configurations are here ſubſtituted as affording a more general, as well as familiar method.——W. P.

O

ſmaller

ſmaller the grand equation of the ſatellites, which depends upon Jupiter's anomaly at the time of rectification.

The laſt moſt favourable time was on October 11, 1794, and the next leaſt favourable will be on July 5, 1800: the former will recur on Auguſt 21, 1806, and the latter on May 17, 1812; and two of each will follow at nearly ſix years diſtance from each other in each revolution of Jupiter afterwards.

If the diameters of the cyphers in the nautical almanac, which are too ſmall, were ſo far augmented as to bear an * exact proportion to the greateſt elongations of the ſatellites there exhibited, which appears to be not the caſe, their apparent places, meaſured by thoſe diameters, might be eaſily aſcertained without further trouble on any given day, as well as their true places of mean motion at the ſuitable times above ſpecified.

The configuration fig. 7. is for half paſt ſix P. M. on October 11, 1794, where it appears, from what has been already remarked, that the firſt ſatellite is approaching Jupiter in its ſuperior ſemicircle near its greateſt weſtern elongation; the ſecond receding in its inferior; and the third and fourth receding each in its ſuperior; the third being near its greateſt eaſtern elongation. The correſponding places of the ſhadows on the ſcreen, when the ſatellitian is rectified for this time, will appear as in fig. 8.

The ſhadows may be made to fall a little higher in their inferior ſemicircle than in their ſuperior, by giving the inſtrument a ſmall reclination with a thin wedge placed under the end next the candle, or by an adjuſting ſcrew preſſing againſt the table, which effect is greateſt with the fourth.

In this repreſentation the ſatellites are very nearly in their true places of *mean* motion, as well as in their *apparent* places: therefore, if the ſatellitian be rectified for Saturday October 11, 1794, at half paſt ſix in the evening, by this latter configuration, and the handle be turned till the month-hand comes to the end of February 1796, before it be put back a day, and thence forward to the preſent time (1798), provided the firſt and ſecond arms be alſo adjuſted for their errors in motion, as already directed, the rectification will be more accurate for *mean* motion at preſent than if made by the almanac at any other time.

From 1794 to 1800 the difference between mean and apparent motion of the ſatellites, rejecting the ſmaller equations, is increaſing; and from 1800, when it will be a maximum, to 1806, it will decreaſe, the apparent being faſter than the mean, according to the year for which the ſatellitian is to be uſed: from 1806 to 1812 it will again increaſe, after which time a decreaſe will commence; and, in a little leſs than every ſix years, the increaſe and decreaſe will continue to be alternate for the time to follow.

Laſtly: Put the index of the candleſtick to the ſun's place in the ecliptic, and the heliocentric longitude of Jupiter taken from a nautical almanac or White's Ephemeris, immediately between the central ſtem and Jupiter in the machine, by means of a thread ſtretched and tied to both, which will ſerve as an index, as well as a guide for the diſtance of the candleſtick;

* The diameter of Jupiter is repreſented in the oppoſite extreme in the Encyclopædia Britannica, both in fig. 18. plate 62. and in fig. 177. plate 79. though it is ſaid, in vol. ii. part 2. p. 577. that "the orbits of Jupiter's moons are drawn in true proportion to his diameter" in the latter diagram, and are evidently intended to be ſuch in the former.——W. P.

candlestick; and in this situation the representatives of the Sun, Earth, and Jupiter, as also of the satellites, will all be in their relative situations for exhibiting the general phenomena of the Jovian as a detached system.

But before I give a particular description of these phenomena as they will be exhibited by the satellitian, the reader will perhaps form a more accurate conception of the extent of its application, if a summary account be first given of those minute irregularities in the motions of the satellites, which no simple machinery can be supposed to represent.

The satellites of Jupiter were discovered by Galileo in January 1610, and called *Medicean stars*, in honour of Cosmo Medici great duke of Tuscany. This astronomer continued his observations upon them for 27 years, till unfortunately the loss of sight frustrated the fruit of his continued labours. In 1663 Borelli published a theory of the Medicean Stars, but had not acquired data sufficient for ascertaining the exact quantities and qualities of their motions.—After him Cassini, in the year 1668, favoured the world with "Tables of the Motions of Jupiter's Satellites," which were improved and edited by him again in 1693. These Tables have since that time been rendered still more accurate by Messrs. Hadley, Pound, Bradley, &c. as also by the French astronomers, and lastly by Wargentin the Swedish astronomer, whose best Tables are now used in calculating the immersions and emersions given in the nautical almanac.—The times of the mean periodical revolutions have been already mentioned; but they are subject to such inequalities of motion as require the following equations for ascertaining their apparent places: viz.

1. For the light which depends upon Jupiter's eccentricity: 2. For the light which depends upon his change of place in his orbit: 3. For his anomaly: 4. For the mutual gravitation of the three first, the period of which is upwards of 437 days: 5. For a period of 12 years for the third, accruing from an unknown cause: 6. For a similar period for the fourth, accruing from its eccentricity: 7. For the variable inclination of the orbit of the second; and, 8. for apparent time.—The greatest or grand equation, which depends upon Jupiter's anomaly, has been shewn to be different in different years: with the first satellite it is possible for it to amount to 1 h. 18 min. 16 sec.; with the second, to 2 h. 37 min. 12 sec.; with the third, to 5 h. 16 min. 32 sec.; and with the fourth, to 12 h. 20 min. 34 sec. at a certain time in some particular years above specified, though in others it may be nothing.

From measurements of the greatest elongations of these satellites, taken by a micrometer, it does not appear certain that their orbits are elliptical, except that of the fourth: the others, however, are by analogy supposed to be such, though the ellipses are so like circles that they may be considered as such, even in calculations, without any apparent error. The diameters of these orbits, when viewed from the earth, subtend but very small angles; viz. the first subtends 3′ 55″; the second, 6′ 14″; the third, 9′ 58″; and the fourth, 17′ 30″.—The diameters of each of the satellites themselves, compared to that of Jupiter, seem not to be accurately known. Mr. Herschell's observations make that of the first to be not quite $\frac{9}{10}$ of a second; whereas, before his notice, they were each considered to subtend an angle more than double this quantity; for their diameters were estimated at $\frac{1}{18}$ or $\frac{1}{20}$ of Jupiter. Future observations must determine this point.

In obferving the eclipfes of Jupiter's fatellites by his fhadow, it has been found that the duration of an eclipfe of each is longer at fome times than at others: that in fome inftances a fatellite paffes through the centre of the fhadow, which is fuppofed to be a little conical, but at others through only a chord of its circular fection; hence an inclination of each of their orbits to that of Jupiter has been proved, and calculated to be as follows: Of the firft, about 2° 55′, the afcending node being at reft near the middle of Aquarius; of the fecond, variable from 2° 50′ (in 1668), to 3° 52′ (in 1715), the afcending node being at reft about 5° of Aquarius; of the third, variable from 3° (in 1695), to 3° 24′ (in 1765), its afcending node being about 25° 58′ of Aquarius at this time (1798), and moving forwards eight minutes in a year; and of the fourth, about 2° 40′, which is very little variable, the afcending node being about the middle of Aquarius.

The apojove of the fourth is at about 24½° of Aries (1798), and moves forward about 3° in five years.

The duration of an eclipfe is the greateft at the nodes and fmalleft at the limits: that of the firft varies from 2 h. 16 min. to 2 h. 7 min. 40 fec.; of the fecond, from 2 h. 51 min. 20 fec. to 2 h. 13 min. 4 fec.; of the third, from 3 h. 35 min. 40 fec. to 1 h. 2 min. 32 fec.; and of the fourth, from 4 h. 46 min. to 0 h. 0 min. 0 fec.

The two oppofite points of the ecliptic, cut by the plane of each orbit extended, are called the geocentric nodes; and a fatellite appears to move in an exact ftraight line only when the earth is in one of thofe; for at other fituations of the earth the track of each appears, though in a fmall degree, elliptical, and the more fo the farther the earth is removed from their nodes: this is moft apparent with the fourth, notwithftanding the nodes are all in the fame figns, by reafon of the greatnefs of its orbit; for, when near either limit, it entirely efcapes an occultation, nor is eclipfed if removed above 52° from either node. This happens to be the cafe this year, and will continue fo nearly, if not quite, throughout the next; and recurs for nearly two years and a half in every fix.

The line in which the fatellites appear is nearly horizontal, as on the fcreen, when Jupiter is on the meridian, but becomes the more oblique the farther he is removed therefrom.

The greateft part of thefe inequalities and peculiarities of motion, as well as their rotations, will not be attempted to be illuftrated by the fatellitian, but fuch phenomena only as are demonftrable to the eye of an obferver independently of calculations, and which therefore may be confidered as the moft proper fubjects of illuftration by machinery.

After having given a defcription of the fatellitian and its appendages; of the method of rectifying it for ufe; and of the principal minutiæ relating to the motions of the fatellites; I come now, in the laft place, to particularize thofe phenomena to be illuftrated, which a telefcope of a moderate magnifying power for celeftial objects will prefent to the obfervation of a fpectator, and which afford a perpetual fource of amufement to any perfon who is in poffeffion of a good inftrument. It is neceffary however to make this previous remark, that if the telefcope invert the object, the pofterior furface of the fcreen muft be viewed; but if it fhew it direct, the anterior will be proper.

When the fatellitian is properly rectified, the fcreen fixed and marked, and the candleftick adjufted with only one candle, the central one being removed, each turn of the handle

handle will produce one day's motion of every ſatellite; and a continuation of ſlow regular turns will produce a pleaſing view of the following phenomena; viz.

1. The ſhadows will move in nearly a ſtraight line.

2. Some in a direct and others in a retrograde direction.

3. The moſt diſtant will frequently appear the neareſt to Jupiter.

4. Near Jupiter they will move the quickeſt, and ſloweſt near their greateſt elongation, where they become ſtationary for a ſhort time.

5. Their greateſt elongations will be a little before quadrature, when weſt; and after, when eaſt of Jupiter.

6. Hence it will appear, that the ſuperior portion of each orbit is greater than the inferior.

7. Whether to the eaſt or weſt of Jupiter, their motions will be direct in the ſuperior, and retrograde in the inferior, parts of their orbits.

8. When paſſing between Jupiter and the earth (candle) they will tranſit him.

9. When paſſing the ſame line in their ſuperior ſemicircle they will ſuffer an occultation.

10. When paſſing through the four concentric circles they will be eclipſed.

11. The *mean* or *apparent* times, accordingly as the inſtrument may be rectified, of each of theſe phenomena, will be pointed out by the hour-hand, the name of the day by the week-hand, and the day of the month by the month-hand, in each year.

12. If the candleſtick is adjuſted frequently for the ſun's place and Jupiter's heliocentric longitude, the ſolar ſhadow will appear to alter its ſituation to the right and left of Jupiter, as he approaches to or recedes from conjunction or oppoſition.

13. The reaſon will be evident, from the concentric circles, why an immerſion or ingreſs into Jupiter's ſhadow, and a ſubſequent emerſion or egreſs out of it, never both happen with the firſt and * ſecond ſatellites; nor with the third, if Jupiter is within 46° of oppoſition to, or conjunction with, the ſun; nor with the fourth, if that diſtance is leſs than 24°.

14. It will be likewiſe evident why an immerſion only is viſible of the firſt and ſecond ſatellites from a conjunction to an oppoſition (fig. 6.), why an occultation and eclipſe may be coincident when at oppoſition (fig. 5.), and why an emerſion only is ſeen from an oppoſition to a conjunction (fig. 4.).

15. If a proper reclination be given to the ſatellitian in this and on the next year, and alſo on every ſixth and ſeventh year hence, the fourth ſatellite will neither be eclipſed nor ſuffer an occultation.

16. If a candle be now placed in each ſocket of different lengths, as is repreſented in fig. 3. the ſhadow of the central one to cover the concentric circles, there will be two rows of ſhadows on the ſcreen above one another, one of which will repreſent the heliocentric, and the other the geocentric, places of the ſatellites.

* An immerſion and ſubſequent emerſion may be ſeen of the ſecond ſatellite, provided it be near one of its limits at the ſame time that Jupiter is near both his perihelion and quadrature with the ſun; but this will very rarely happen.—W. P.

17. Hence

17. Hence it will appear, that an eclipſe ſeen from the earth is an occultation ſeen from the ſun.

18. That an eclipſe is never viſible at the ſun.

19. And that the ſhadow of a ſatellite, as ſeen from the earth, falls on Jupiter ſometimes before, ſometimes after, and ſometimes at its tranſit, according to the relative ſituations of Jupiter, the ſun, and earth.

20. If the central candle only be ſuffered to remain, and the little balls themſelves be viewed in an oblique direction, the reaſon will be apparent why the ſatellites are ſeen lunated from Jupiter:

21. And alſo Jupiter lunated when ſeen from them;

22. But neither of them lunated when ſeen from the earth or ſun.

23. It will alſo be evident why the ſun is frequently eclipſed to the inhabitants of Jupiter by the ſhadows of his ſatellites.

24. And, laſtly, why the ſatellites, as ſeen from the earth, do not eclipſe one another.

Theſe, and perhaps other phenomena not ſpecified here, will be illuſtrated by the ſatellitian in ſo diſtinct a manner, as to convey to a ſpectator, who may compare the apparent motions of the ſhadows to the real motions of the little balls, a clear conception of the relation that the apparent motions of Jupiter's ſatellites, as viewed from the earth with a teleſcope, have to their true circular motions, as viewed by the inhabitants of Jupiter. But though a clear conception may thus be formed by a ſpectator of the true and apparent motions of the ſatellites as they regard Jupiter himſelf, yet he muſt be informed that the *real track* in which they move round the *ſun* along with Jupiter, the progreſſive centre of their detached ſyſtem, is neither circular nor elliptical, but in a line which croſſes Jupiter's track in a ſinuoſe manner, more or leſs frequently as their periods are ſhorter or longer. As the velocity of the firſt and ſecond ſatellites exceeds the velocity of Jupiter, they are not only apparently, but really retrograde in ſome part of every revolution; on which account their tracks form loops which are alternately concave and convex towards the ſun; the concavity being greater than the convexity: but as the velocity of Jupiter exceeds that of the third and fourth, theſe are not really but only apparently retrograde at the inferior ſemicircles: on this account their tracks are always concave, except at their ſtationary points, which project and divide the concave ſpaces. Theſe real tracks will eaſily be comprehended, if, while the ſatellites are in motion, Jupiter himſelf be alſo conceived to have a direct motion ſlower than the two firſt, but quicker than the two laſt.

If now, after what has been ſaid, we conceive ourſelves, like the philoſophical poet *, conveyed into the regions of Jupiter and his ſatellites, we ſhall with him find cauſe for adoring the power, wiſdom, and goodneſs of the Almighty Creator! When we conſider that Jupiter, the diameter of which planet is more than ten times larger than that of our earth, has a rotation on its axis in the ſmall ſpace of nine hours and fifty-ſix minutes, we

* ——— "remote from day's all-cheering ſource,
"Large Jupiter performs his conſtant courſe:
"Four friendly moons with borrow'd luſtre riſe,
"Beſtow their beams benign, and light his ſkies." BAKER's *Univerſe*.

must perceive, that, without the assistance of some other luminary besides the sun, total darkness would be the lot of his inhabitants for nearly five hours in every ten.—Again, if we consider that Jupiter's year, or periodical revolution, contains 4332 d. 8 h. 51½ min. of our time, or 22936,43+ of his own days, and that he has no sensible change of seasons, by reason of the nearly perpendicular direction of his axis, the inclination of which is only 1° 20′, we must see that the number of his days in his year might frequently be lost, without some intermediate remembrancers between those two very distant extremes: accordingly, the Omnipotent Father of the creation has provided a remedy for these inconveniences: he has furnished Jupiter with these four moons to cheer his inhabitants with light in the frequent absence of the sun's rays, and to present them with four different kinds of months, like our years, months, and weeks.

Of the first kind of months in Jupiter's year there are 244,97+, each of which contains 9,36+ of his days; of the second kind there are 121,89+, each containing 18,81+; of the third kind there are 60,45+, each comprising 37,94+; and of the fourth kind there are only 25,85+, each of which comprises 88,68+ of his days; so that each shorter month, particularly of the three first kinds, is very nearly *double* the next longer; and may be considered as exactly such, in counting time by divisions and subdivisions of months, by the help of intercalary days.

But besides these uses of Jupiter's satellites, and others, perhaps, which our limited capacities can never comprehend, there are three very considerable advantages, which the inhabitants of our globe possess, accruing from the observations which have already been made upon them; viz. an accurate knowledge of the parallax of the earth's annual orbit; of the real velocity of light; and of an easy method of ascertaining the longitude of places by land; all which are explained in the different books upon astronomy:—so true is it throughout the whole grand scale of nature, that no individual part is without its utility.

Lincoln,
March 10, 1798.

VI.

*Objections to the Opinion of Professor Spallanzani respecting the Cause of the Light of Natural Phosphori. Communicated to Mr. John Fabbroni, Sub-Director of the Royal Museum of Florence. By M. Joachim Carradori, M. D.**

AT length the hypothesis of Goettling is entirely destroyed, in consequence of the refutations of various celebrated authors, and among them Spallanzani. You may perhaps recollect that I foretold its short duration, and I pointed out to Brugnatelli, at its first appearance, several proofs of its falsity, which might be deduced from the very experiments it was built upon.

Though the refutation of Spallanzani † has not the merit of being the first, its excellence

* Annales de Chimie, xxiv. 216.

† Chimico Esame del Cittadino Spallanzani. Modena. 1796.

cannot

cannot be diſputed. I have read this ſhort treatiſe with pleaſure, from the many excellent and amuſing points of knowledge it contains. But on peruſing it with a certain degree of attention, I cannot avoid making ſeveral obſervations which the facts appear to demand.

It appears to me that Spallanzani ſuppoſes himſelf to be the firſt who obſerved that water has the property of abſorbing oxygen from the air of the atmoſphere. For at the 114th page of his book, he ſays: Trovai pertanto che l'acqua e' un mezzo di decomporre l'arie, come lo ſono il fosforo di Kunckel e i ſulfuri alcalini; mà ella agiſce con eſtrema lentezza. "I found, however, that water is a medium for decompoſing the air in the ſame manner as Kunckel's phoſphorus, or the alcaline ſulphurets; but it acts with extreme ſlowneſs."—But this fact was already known to Scheele (Treatiſe of Air and Fire), and conſequently the honour of this diſcovery is his right. He was the firſt who obſerved that, by keeping a bottle of atmoſpheric air inverted over water for ſeveral days, the water gradually riſes; the volume of air is diminiſhed, and azotic gas, or, as it was then called, phlogiſticated air, remains alone. It is ſurpriſing that Spallanzani ſhould not be aware of this; or, if he knew it, that he ſhould have omitted mentioning it.

I have very important facts to ſtate againſt his theory of natural phoſphori. It is long ſince I firſt obſerved that the phoſphoric wood not only ſhines under water and under oil, but even in the barometric vacuum; which obſervations are related in the ſecond volume of my theory of heat. If the phoſphoric wood require air to enable it to ſhine, how can it continue to emit light under oil, where the vital air neither exiſts nor can have acceſs? I muſt likewiſe remark the difference which Spallanzani has obſerved between the urinous phoſphorus and that of wood; namely, that when the former is entirely ſurrounded by any pure mephitic air, it immediately ceaſes to give light; whereas the other being placed in pure azotic air continues to ſhine during ſix minutes, and does not entirely loſe its light till half an hour afterwards.

When Spallanzani introduced phoſphoric wood into vital air, or oxygen gas, how did it happen that he omitted to obſerve, whether by its ſhining in that fluid for a conſiderable time, there was no diminution of volume, as he obſerved that this diminution took place when the phoſphoric flies (lucciole) were placed therein?

The luccioloni, or glow-worms, as well as the lucciole, ſhine under oil. I have obſerved a luminous fly continue to ſhine perfectly for a quarter of an hour in the barometric vacuum.

Spallanzani found that the phoſphorus of theſe flies ſhines much more in oxygen gas than in the air of the atmoſphere, and is totally extinguiſhed by air not capable of maintaining combuſtion. But how can we explain their ſhining under oil for hours together? The fact is certain, becauſe I have repeated the experiment with many variations; ſometimes by putting the phoſphoric flies entire beneath the oil, and ſometimes the phoſphorus only detached from the inſect, and even cruſhed.

This obſervation muſt be well known to you, ſince I communicated it laſt year to the Royal and Oeconomical Society of Florence, in my Memoir on the Lucciole; and it will be equally known to the whole ſcientific world, as it will be publiſhed in the xiiith volume of the Chemical Annals of the celebrated Brugnatelli.

The

The experiment of placing the luminous flies in oxygen is not new. It was before made by Forster; for which see his Theory of Heat, and the Journal de Rozier for 1784. He observed that they give much more heat in this fluid, not only at intervals, but continually. M. Forster moreover assures us, that he discovered the organs of respiration in those insects, which M. Spallanzani could not find. He describes them precisely, by observing that in each of the luminous rings there are two air passages, furnished with valvules at their apertures, and that these passages are lost in the interior structure of the animal.

I have seen two species of luccioloni, or shining worms; namely, a larger and a smaller, of which I have given a slight description in my Theory of Heat, where I speak of phosphori. The first species also differs from the second in colour; the former being nearly grey, and the latter nearly black. The first have much luminous matter in the three last rings; and the others have less in the last ring but one. These may perhaps have been the species observed by the Naturalist of Pavia. Both species conceal their phosphorus at pleasure with the utmost facility. They seem therefore to be aware of its effects, particularly the second species.

It is not true, as certain naturalists, as well as M. Spallanzani, pretend, that the luccioloni and lucciole are animals of the same species, different only in sex, that is to say, that the former are the males, and the latter the females. I can affirm that I have seen the lucciole pregnant, with the abdomen filled with eggs: and this part, which before was so luminous, had its phosphoric part very much diminished, being reduced to merely two points, and two small lateral portions. The lucciole begin to disappear precisely at the time of their pregnancy, which is the cause why they conceal themselves. If they be sought for among the grass and underwood, they are found in this state*.

If it be certain then that phosphoric wood, the luccioloni and the lucciole continue to shine under oil, it must be admitted that this light does not arise from a slow combustion, as Spallanzani pretends, because oil contains no air to support it. This is the legitimate and immediate consequence which must follow from this experiment, and cannot be refused. The experiments of Spallanzani lead to peculiar consequences, though different from those he has deduced. It is possible that the unrespirable air may act in a particular manner on these phosphori, which may be capable of preventing the emanation of their lights; and that oxygen, by a particular action of a contrary kind, may augment it.

Why should it be disputed that the different airs may produce peculiar effects on these substances, with which we are not yet acquainted? To Spallanzani will be due the honour of first observing them. In the same manner as various fluids are pernicious to natural phosphori, and prevent their shining, why may not the air produce the same effect? I have found by experiment, that the phosphorus of the lucciole is suddenly extinguished if they be plunged in alcohol, or in vinegar; but continues to shine in oil in the same manner as in water and in air.

It may be objected to me, that Spallanzani has made the experiment, that the phosphorus of the shining worms produces a diminution of volume in oxygen gas, at the same time that their light is increased; whence it ought to be concluded that the process is analogous

* See the note No. 1. p. 78. tom. ii. of my Theory of Heat. C.

to combuſtion. But this conſequence is not certain. How many ſubſtances are there which have the property by their emanations of altering vital air, in the ſame manner as combuſtion, which nevertheleſs do not burn nor emit light? The ſame may be the caſe with the phoſphoric matter of theſe inſects.

There is not a perfect analogy, as Spallanzani pretends, between the phoſphori he has obſerved and the urinous phoſphorus, becauſe the latter does not ſhine but at a certain heat; whereas the natural phoſphori ſhine at any temperature whatever, provided it be not ſo great as to alter their ſubſtance. This proves, in my opinion, that the light is not an effect of combuſtion; for every combuſtion requires a more or leſs elevated degree of heat.

With regard to the explanation given by Spallanzani of the change of wood into the luminous matter, which ſuppoſes that the hydrogen and carbon, being ſet at liberty, attract oxygen; I will venture to ſay, on the ſtrength of my experiments, that it is not probable. It is certain (for I have obſerved it myſelf, and probably ſome others before me), that wood, when luminous, has almoſt totally loſt its reſinous part, and that conſequently in that ſtate it retains ſcarcely any either of the carbonic or hydrogenous principle*, to which its combuſtibility was owing. And in fact, ſuch wood as has become luminous is with difficulty burned in the fire, and produces no flame, as every one may try. Indeed it cannot be otherwiſe; for the progreſs of putrefaction, which reduces it to that ſtate, muſt have deprived it of much of its component parts; particularly the moſt volatile, ſuch as hydrogen. Beſides which, as I obſerved ſome months ago to Brugnatelli, I think very differently from Spallanzani with regard to the conſtitution of theſe ſubſtances; namely, that they become phoſphoreſcent in proportion as they have loſt their inflammable principle, and that the property of abſorbing and retaining the light depends on that circumſtance.

My opinion may be extended, in preference to that of Spallanzani, to the cauſe of the phoſphoric property of animals; for it is more reaſonable: becauſe we cannot imagine that their luminous matter is either reſinous or oily, and conſequently is not inflammable; neither can it contain much carbon or hydrogen. See my Memoire on the Lucciole, Ann. Chim. et Hiſtoriques de Pavie, tom. xiii.

If the lucciole ſhine beneath the water, as Spallanzani maintains, becauſe the oxygen gas contained in water ſerves to maintain their combuſtion, why does not the phoſphorus of Kunckel alſo ſhine beneath that fluid? It was likewiſe neceſſary to have exhibited ſome experiments in ſupport of that opinion; as for example, to have ſhewn that the phoſphorus of thoſe animals alters or abſorbs the vital air contained (uncombined) in water, and that water which does not contain it is not capable of cauſing them to ſhine.

Pavia,
April 15, 1797.

* Of what principles, then, is this vegetable reſidue compoſed? N.

VII. *Sketch*

VII.

Sketch of the Hiſtory of Sugar, in the early Times, and through the Middle Ages (a).

By W. FALCONER, *M.D. F.R.S. &c. &c.*

THE uſe of ſugar is probably of high, though not remote antiquity, as no mention of it is made, as far as I can find, in the ſacred writings of the Old Teſtament (*b*). The conqueſts of Alexander ſeem to have opened the diſcovery of it to the weſtern parts of the world.

Nearchus *, his admiral, found the ſugar cane in the Eaſt Indies, as appears from his account of it, quoted by Strabo (*c*) It is not, however, clear, from what he ſays, that any art was uſed in bringing the juice of the cane to the conſiſtence of ſugar.

Theophraſtus †, who lived not long after, ſeems to have had ſome knowledge of ſugar, at leaſt of the cane from which it is prepared In enumerating the different kinds of honey, he mentions one that is found in reeds (*d*), which muſt have been meant of ſome of thoſe kinds which produce ſugar.

Eratoſthenes ‡ alſo is quoted by Strabo (*e*), as ſpeaking of the roots of large reeds found in India, which were ſweet to the taſte both when raw and when boiled.

The next author, in point of time, that makes mention of ſugar, is Varro §, who, in a fragment quoted by Iſidorus (*f*), evidently alludes to this ſubſtance. He deſcribes it as a fluid, preſſed out from reeds of a large ſize, which was ſweeter than honey.

Dioſcorides (*g*) ‖, ſpeaking of the different kinds of honey ſays, that "there is a kind of "it, in a concrete ſtate, called *saccharon*, which is found in reeds in India and Arabia "Felix. This, he adds, has the appearance of ſalt; and, like that, is brittle when chewed. "It is beneficial to the bowels and ſtomach, if taken diſſolved in water; and is alſo uſeful "in diſeaſes of the bladder and kidneys. Being ſprinkled on the eye, it removes thoſe

(*a*) Mancheſter Memoirs, iv. 291.

(*b*) Since writing the above, I have obſerved that the *ſweet cane* is mentioned in two places of ſcripture, and in both as an article of merchandize. It does not ſeem to have been the produce of Judea, as it is ſpoken of as coming from a far country. Iſaiah, chap. xliii. v. 24. Jeremiah, chap. vi. v. 20.—It is worthy of remark, that the word *sachar* ſignifies, in the Hebrew language, inebriation, which makes it probable that the juice of the cane had been early uſed for making ſome fermented liquor.

(*c*) Ειρηκε δε περι των καλαμων οτι ποιουσι μελι, μελισσων μη ουσων. Strabon. l. xv.

(*d*) Αλλη δε εν τοις καλαμοις. Fragment of Theophraſtus preſerved in Photius. See p. 864. edit. Augſburg. 1601.

(*e*) Και τας ριζας των φυτων, και μαλιστα των μεγαλων καλαμων, γλυκειας και φυσει και εψησει. Strabon. l. xv.

(*f*) Indica nam magna nimis arbore creſcit harundo;
Illius e lentis premitur radicibus humor,
Dulcia cui nequeant ſucco contendere mella. Iſidor. lib. xvii. cap. 7.

(*g*) Eſt et aliud concreti mellis genus, quod saccharon nominatur. In Indiâ vero et Felici Arabiâ, in harundinibus invenitur. Salis modo coactum eſt; dentibus, ut ſal, fragile; alvo idoneum et ſtomacho utile, ſi aqua dilutum bibatur; vexatæ veſicæ, renibuſque auxiliatur. Illitum ea diſcutit, quæ tenebras oculorum pupillis offundunt. Matthioli Dioſc. cap. lxxv.

* Ante Chriſt. ann. 325. † A. C. 303. ‡ A. C 223. § A. C. 68. ‖ A. C. 35.

"ſubſtances that obſcure the ſight." The above is the firſt account I have ſeen of the medicinal virtues of ſugar.

Galen * appears to have been well acquainted with ſugar, which he deſcribes, nearly as Dioſcorides had done, as a kind of honey, called *ſacchar*, that came from India and Arabia Felix, and concreted in reeds. He deſcribes it as leſs ſweet than honey, but of ſimilar qualities, as detergent, deſiccative, and digerent. He remarks a difference, however, in that ſugar is not like honey injurious to the ſtomach, or productive of thirſt (*a*).

If the third book of Galen, "*Upon Medicines that may be eaſily procured*," be genuine, we have reaſon to think ſugar could not be a ſcarce article, as it is there repeatedly preſcribed.

Lucan † alludes to ſugar, in his third book, where he ſpeaks of the ſweet juices expreſſed from reeds, which were drank by the people of India (*b*).

Seneca ‡, the philoſopher, likewiſe ſpeaks of an oily ſweet juice in reeds, which probably was ſugar (*c*).

Pliny § was better acquainted with this ſubſtance, which he calls by the name of *ſaccaron*; and ſays, that it was brought from Arabia and India, but the beſt from the latter country. He deſcribes it as a kind of honey, obtained from reeds, of a white colour, reſembling gum, and brittle when preſſed by the teeth, and found in pieces of the ſize of a hazel nut. It was uſed in medicine only (*d*).

Salmaſius, in his Plinianæ Exercitationes, ſays, that Pliny relates, upon the authority of Juba the hiſtorian, that ſome reeds grew in the Fortunate Iſlands, which increaſed to the ſize of trees, and yielded a liquor that was ſweet and agreeable to the palate. This plant he concludes to be the ſugar cane; but I think the paſſage in Pliny (*e*) ſcarcely implies ſo much.—Hitherto we have had no account of any artificial preparation of ſugar, by boiling or otherwiſe; but there is a paſſage in Statius ‖, that ſeems, if the reading be genuine, to allude to the boiling of ſugar, and is thought to refer immediately thereto by Stephens in his Theſaurus (*f*).

Arrian ¶, in his Periplus (*g*) of the Red Sea, ſpeaks of the honey from reeds, called

(*a*) De ſimplic. Medicamentis. Lib. vii.

(*b*) Quique bibunt tenerâ dulces ab arundine ſuccos. Lucani Pharſaliæ lib. iii. lin. 237.

(*c*) Aiunt inveniri apud Indos mel, in arundinum foliis, quod aut ros illius cœli aut ipſius arundinis humor dulcis et pinguior gignat. Senec. Epiſtol. l. i. Epiſt. lxxxiv.

(*d*) Saccaron Arabia fert, ſed laudatius India. Eſt autem mel in arundinibus collectum, gummium modo candidum, dentibus fragile, ampliſſimum nucis avellanæ magnitudine, ad medicinæ tantum uſum. Plin. Hiſtor. Natural. l. xii. cap. viii.

(*e*) Plin. Hiſt. Nat. lib. vi. cap. xxxii.

(*f*) Et quas præcoquit Eboſita cannas
Largis gratuitum cadit rapinis. Stat. Sylv. l. vi. 15.

Haud dubie (inquit Stephanus) cannas intelligit ex quibus ſaccharum exprimitur vel coquitur. Et fortaſſe cannas pro ſaccharo ipſo poſuit. Sed qui Eboſitæ illi, hactenus apud neminem invenimus. Populi fortaſſe ſunt Indiæ, ubi ſaccharum potiſſimum naſcitur. Steph. Theſ. Vox Canna. Lectio autem dubia eſt. Vide Not. Marklandi in hunc locum.

(*g*) Μελι το καλαμινον το λεγομενον Σαχαρι. Page 150. Ed. Amſtelod. 1683, 8vo.

* Anno poſt Chriſt. nat. 143. † Lucani mors, A. D. 65. ‡ Senecæ mors, A. D. 65.

§ Plinii mors, A. D. 77. ‖ A. D. circ. 80. ¶ A. D. 145.

ſacchar (Σαχαρ) as one of the articles of trade between Ariace and Barygaza, two places of the Hither India, and ſome of the ports on the Red Sea.

Ælian *, in his Natural Hiſtory, ſpeaks of a kind of honey, which was preſſed from reeds that grew among the *Praſii*, a people that lived near the Ganges.

Tertullian † alſo ſpeaks of ſugar, in his book *De Judicio Dei*, as a kind of honey procured from canes (*a*).

Alexander Aphrodiſæus (*b*) ‡ appears to have been acquainted with ſugar, which was in his time regarded as an Indian production. He ſays, "that what the Indians called "ſugar, was a concretion of honey, in reeds, reſembling grains of ſalt, of a white colour, "and brittle, and poſſeſſing a detergent and purgative power like to honey; and which "being boiled, in the ſame manner as honey, is rendered leſs purgative, without impairing "its nutritive quality."

Paulus Ægineta (*c*) § ſpeaks of ſugar as growing, in his time, in Europe, and alſo as brought from Arabia Felix; the latter of which he ſeems to think leſs ſweet than the ſugar produced in Europe, and neither injurious to the ſtomach nor cauſing thirſt, as the European ſugar was apt to do.

Achmet (*d*), ‖ a writer who, according to ſome, lived about the year 830, ſpeaks familiarly of ſugar as common in his time.

Avicenna (*e*), ¶ the Arab phyſician, ſpeaks of ſugar as being a produce of reeds; but it appears he meant the ſugar called Tabaxir or Tabarzet, as he calls it by that name.

It does not appear, that any of the above-mentioned writers knew of the method of preparing ſugar, by boiling down the juice of the reeds to a conſiſtence. It is alſo thought, the ſugar they had was not procured from the ſugar-cane in uſe at preſent, but from another of a larger ſize, called Tabarzet (*f*) by Avicenna, which is the *Arundo Arbor* of Caſpar Bauhin, the *Saccar Mambu* of later writers, and the *Arundo Bambos* of Linnæus. This yields a ſweet milky juice, and oftentimes a hard cryſtallized matter, exactly reſembling ſugar, both in taſte and appearance.

The hiſtorians of the Cruſades make the next mention of ſugar of any that have fallen under my obſervation.

The author of the Hiſtoria (*g*) ** Hieroſolymitana ſays, that the Cruſaders found in Syria certain reeds called *Cannameles*, of which it was reported a kind of wild honey was made; but does not ſay that he ſaw any ſo manufactured.

(*a*) Mella viridanti confragrant pinguia canna. Tertullian. de Judicio Dei.

(*b*) Alex. Aphrodiſæi lib. ii. Probl. 79.

(*c*) Paul. Æginetæ Vox Mel. Μελι. p. 632. Medic. Art. Princ. Ed. Henrici Stephani, 1567.

(*d*) Vide Meurſii Gloſſ. Græc. Barb. & Du Cange Gloſſ. ad Script. med. & inf. Græcitatis.

(*e*) De Zuccaro. Lib. II. Tract. II. De Melle. Lib. II. Tract. II.

(*f*) Some of the writers ſay, that it was ſo called from the name of a place Σαχαρ Ταβαρζυς, τοπος ηλω καλ-μευες εις Συριαν. Conſtantinus a Secretis, MS. quoted from Du Cange Gloſſ. Græc. The word Tabarzet ſignifies white, and is tranſlated, by Du Cange, Saccar Album. Herbelot ſays, that the Perſians called by that name the hardeſt and moſt refined ſugar. Bibliothéque Orientale, p. 810.

(*g*) Pars ſecunda, p. 595.

* A. D. circ. 145. † A. D. 195. ‡ A. D. 212.

§ A. D. circ. 400. vel ſecundum Friend multo poſterior. Hiſt. Medic.

‖ A. D. 830. ¶ A. D. 980. natus. ** 1100.

Albertus

Albertus Agnenſis *(a)* * relates, that about the ſame period, "the Cruſaders found ſweet "honeyed reeds in great quantity, in the meadows about Tripoli, in Syria, which reeds "were called *Zucra*. Theſe the people (the Cruſaders army) ſucked, and were much "pleaſed with the ſweet taſte of them, with which they could ſcarcely be ſatisfied. This "plant (the author tells us) is cultivated with great labour of the huſbandmen every "year. At the time of harveſt, they bruiſe it when ripe in mortars; and ſet by the "ſtrained juice in veſſels, till it is concreted in form of ſnow, or of white ſalt. This, "when ſcraped, they mix with bread, or rub it with water, and take it as pottage; and it "is to them more wholeſome and pleaſing than the honey of bees. The people who were "engaged in the ſieges of Albaria Marra and Archas, and ſuffered dreadful hunger, were "much refreſhed hereby."

The ſame author †, in the account of the reign of Baldwin, mentions eleven camels, laden with ſugar, being taken by the Cruſaders, *(b)* ſo that it muſt have been made in conſiderable quantity.

Jacobus de Vitriaco mentions ‡, *(c)* that "in Syria reeds grow that are full of honey, by "which he underſtands a ſweet juice, which by the preſſure of a ſcrew-engine, and con-"creted by fire, becomes ſugar." This is the firſt account I have met with of the employ-ment of heat or fire in the making of ſugar.

About the ſame period § *(d)* Willermus Tyrenſis ſpeaks of ſugar as made in the neighbour-hood of Tyre, and ſent from thence to the fartheſt parts of the world.

Marinus Sanutus mentions *(e)* ‖, that in the countries ſubject to the Sultan, ſugar was pro-duced in large quantity, and that it likewiſe was made in Cyprus, Rhodes, Amorea, Marta, Sicily, and other places belonging to the Chriſtians.

Hugo Falcandus *(f)* ¶, an author who wrote about the time of the Emperor Frederic Bar-baroſſa, ſpeaks of ſugar being in his time produced in great quantity in Sicily. It appears to have been uſed in two ſtates; one wherein the juice was boiled down to the conſiſtence of honey, and another where it was boiled farther, ſo as to form a ſolid body of ſugar.

(a) Calamellos ibidem mellitos, per camporum planiciem abundanter repertos, quos vocant Zucra, ſuxit populus illorum ſalubri ſucco lætatus; et vix ad ſaturitatem prædulcedine explere hoc guſtato valebant. Hoc enim genus herbæ, ſummo labore agricolarum, per ſingulos excolitur annos. Deinde, tempore meſſis, maturum mortariolis indigenæ contundunt, ſuccum colatum in vaſis ſuis reponentes, quouſque coagulatus indureſcat, ſub ſpecie nivis vel ſalis albi. Quem raſum cum pane miſcentes, aut cum aquâ terentes, pro pulmento ſumunt; et ſupra favum mellis guſtantibus dulce ac ſalubre eſſe videtur. His ergo calamellis melliti ſaporis, populus in obſidione Albariæ Marræ et Archas multum horrenda fame vexatus, eſt refocillatus.
Geſt. Dei per Francos, p. 270.

(b) Geſta Dei, p. 353.

(c) Sunt autem calamelli, calami pleni melle ſucco dulciſſimo, ex quo quaſi in torculari compreſſo, et ad ignem condenſato, prius quaſi mel poſthæc quaſi Zuccara efficitur. Geſt. Dei, p. 1075.

(d) Per inſtitores ad ultimas orbis partes deportatur. Geſt. Dei, p. 835.

(e) Marin. Sanut. L. I. Part I. Cap. 2.—in parte ſecunda Geſt. Dei.

(f) In Præfatione ad Libr. de Calamitatibus Siciliæ.

* 1108. † 1110. ‡ 1114. § 1124. ‖ 1306. ¶ 1170.

The foregoing are all the paſſages that have occurred to my reading on this ſubject. They are but few and inconſiderable, but may ſave trouble to others, who are willing to make a deeper enquiry into the hiſtory of this ſubſtance.

Jan. 24, 1790.

The following paſſage, taken from the Viridarium Franciſci Mendozæ, Sacræ & Profanæ Eruditionis. Coloniæ Agrippinæ, 1633, ſeems to point out, though rather obſcurely, the conſtruction and principles of Balloons.

"Vas æreum, plenum aëre, aliter demergendum, in ſumma aqua ſuſtentatur, cum ea ſit naturaliter multo gravius; ergo navis lignea, aut cujuſcunque alterius materiæ in ſummâ aëris ſuperficie conſtituta, et elementari igne repleta, ſupra aërem ſuſtinebitur, nec prius in ipſo aëre ſubmergetur, quam navigii gravitas ſuperet levitatem ignis, quo plenum eſt."

Problema XLVII. Utrum aër parte aliquâ ſit navigabilis. W. F.

VIII.

Experiments and Obſervations on the Effect of Annealing a Plate of Metal, conſiſting of fine or alloyed Gold, with one twenty-fourth Part of Tin. By* MATHEW TILLET.

THOUGH the advantage be great, that men of enlightened minds and well known artiſts ſhould make inquiries into the kind of works to which their attention has been particularly applied, and ſhould render their obſervations public; it is nevertheleſs true, that their reputation, in many reſpects well founded, does in ſome inſtances cauſe the reſults drawn from their experiments to be too ſpeedily adopted; and that when they are carefully repeated, it does not always happen that they prove exact. Theſe artiſts, no doubt, well informed and faithful in their operations and reports, have not ptetended to offer them as deciſive; but by neglecting to conſider the facts before them in every point of view, they have too ſpeedily concluded that theſe facts, univerſally conſidered, were ſuch as to them they appeared at firſt ſight.

Looking over the Journal de Phyſique for the month of September 1788, I ſaw a paper entitled "Experiments and Obſervations on the Fuſion of Gold with Tin," which I read with great attention, as being the production of a man of merit well verſed in his art, namely. Mr. Alchorne, aſſay-maſter to the Engliſh mint. As the object of his experiments is intereſting to every artiſt who uſes gold and ſilver; as its aim is to remove their fears reſpecting the mixture of a certain quantity of tin with fine or alloyed gold, and as theſe artiſts do nevertheleſs retain their apprehenſions with regard to the ſmalleſt mixture of tin with the gold of various fineneſs, which they are inceſſantly melting; I have thought it proper to repeat the experiments of Mr. Alchorne: and while I admit part of the facts he has related, I muſt refute thoſe which my own trials ſhew to be erroneous, and which, if they had not eſcaped his attention, would doubtleſs have led him to make certain exceptions, in the too poſitive conſequences he has drawn from his operations.

Before I enter upon a detail of the experiments of Mr. Alchorne, as well as my own, I think it proper to take notice of the manner in which the editor of the Journal announ-

* Memoirs of the Academy of Sciences at Paris for the year 1790, being the concluding volume, printed in 1797.

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ces the memoir in queſtion. He appears to be convinced that the world had adopted an erroneous opinion reſpecting the object of the paper previous to its appearance; whence it follows, that this Journal, which is deſervedly eſteemed, and of wide circulation, cannot fail to produce a ſtrong impreſſion in this reſpect, which ought to be contradicted. It is in fact founded on operations which have not been purſued with ſufficient accuracy; and on this account they tend to produce a falſe ſecurity in the mind of artiſts who work the moſt precious of metals.

It has long been a received fact among metallurgiſts, obſerves the editor, that tin mixed with gold in the ſmalleſt quantity, either in ſubſtance or in vapour, is totally deſtructive of the malleability of that metal. But Mr. Woulfe, fellow of the Royal Society of London, communicated to that Society, in 1764, a memoir of Mr. Alchorne, aſſayer at the Mint in the Tower of London, and ſince printed in the Philoſophical Tranſactions, in which he proves, that tin may be mixed with gold in a moderate quantity without producing theſe bad effects. Theſe experiments, he adds, have not been contradicted; notwithſtanding which, ſeveral of the moſt celebrated authors have continued to follow the ancient opinion, though in no reſpect founded upon fact. It may therefore be ſuppoſed, that the memoir of Mr. Alchorne has not been ſufficiently known and attended to; for which reaſon I have thought proper to give an abſtract, to make it more generally known.

M. Alchorne relates, that he had long doubted this extraordinary property attributed to tin; and that an opportunity having offered, he made various experiments on that ſubject. He mixed twelve ounces of fine gold with different proportions of tin from ſixty grains to half an ounce. Theſe compounds were beaten under the hammer, paſſed through the laminating rollers, and ſtruck in the fly-preſs without ſhewing any brittleneſs. He likewiſe attempted to expoſe gold to the vapour of tin; for which purpoſe he put twelve ounces of gold of 22 carats into a ſmall crucible, which he placed in a larger crucible, and ſurrounded it with tin, and ſubjected the whole to a conſiderable heat for half an hour; but the gold loſt nothing of its ductility. He carried his reſearches ſtill farther: he alloyed the foregoing mixtures with copper, and afterwards added tin to the gold thus alloyed with different proportions of copper and ſilver; but in all the various caſes, twelve ounces of gold alloyed with tin in the quantity of half an ounce, and of copper two ounces and a half, ſuffered hammering and laminating to the thickneſs of ſtrong paper, and could be wrought into ſmall toys and drawn into fine wire with the ſame facility as the gold of commerce.

M. Alchorne obſerves, that the old opinion adopted by ſo many authors owes its origin probably to the arſenic which tin commonly contains, as he found that twelve grains of that ſemi-metal in regulus rendered the ſame number of ounces of gold brittle. Whence he concludes that tin, like the other baſer metals, does not injure gold, but in proportion to the quantity of arſenic it contains, and that there is nothing in tin which can deprive gold of its qualities, as was before obſerved.

From this ſhort account of the experiments and obſervations of M. Alchorne, we ſee that he affirms that gold, whether pure or alloyed, being fuſed with tin in the proportion of one part of the former to twenty-four of the latter, forms an alloy which preſerves ductility ſufficient to endure hammering and laminating to the thickneſs of ſtrong paper, to be uſed in toys and drawn into fine wire with the ſame facility as the common ſtandard gold.

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Though I was perfuaded that tin deprived gold of its great ductility, or at leaft rendered it fo brittle that it could not be reduced to thin leaves, nor more efpecially be made to pafs the wire plate but by virtue of repeated annealing, and peculiar treatment which gold of the ufual ductility does not require, I neverthelefs determined to repeat the experiments of Mr. Alchorne, as well from efteem for that fkilful artift, as to fupply fuch facts as might appear to have efcaped his notice.

My firft experiment confifted in mixing 24 grains of fine gold with one of tin, taken from an ingot of this laft metal which contains no arfenic. I wrapped this grain of tin in the 24 grains of gold reduced to a very thin leaf, rendered very flexible by annealing. I placed thefe 25 grains upon a piece of charcoal hollowed out, upon which they could be fupported during their fufion. I even fprinkled a fmall quantity of calcined borax upon the metal, in order that the fufion might be more fudden, that the metal might flow together, and the tin unite with the gold without allowing time for it to become calcined. This alloy was fpeedily fufed by the enameller's lamp, and reduced into a fmall button without any lofs of weight. It was then flattened carefully beneath the hammer; but, notwithftanding my precaution in this refpect, it cracked, and at laft broke into three pieces, its thicknefs then being a quarter of a line or thereabouts.

I repeated this firft experiment with a double quantity as well of pure gold as of tin. The refult was the fame. This fecond button was brittle, and likewife broke under the hammer, though I had carefully managed the procefs of hammering in order that this button might have continued whole notwithftanding the cracks.

It is eafily feen that thefe experiments, which were in fome meafure preparatory, tended to intimate the confequences I had reafon to expect when I fhould repeat them more at large, and in a manner more nearly refembling the experiments with which I meant to compare them.

[*To be concluded in our next.*].

ACCOUNTS OF BOOKS.

Philofophical Tranfactions of the Royal Society of London, for the Year 1798. Part I. Quarto. 199 pages, with 26 pages of Meteorological Journal, and 7 plates. Sold by Elmfly, London.

THIS part contains the following papers: 1. The Bakerian Lecture. Experiments upon the Refiftance of Bodies moving in Fluids. By the Rev. Samuel Vince, A.M. F.R.S. Plumian Profeffor of Aftronomy and Experimental Philofophy in the Univerfity of Cambridge.—2. Experiments and Obfervations, tending to fhew the compofition and properties of Urinary Concretions. By George Pearfon, M.D. F.R.S.—3. On the Difcovery of four additional Satellites of the Georgium Sidus; the retrograde motion of its old Satellites announced, and the caufe of their difappearance at certain diftances from the planet explained. By William Herfchel, L.L.D. F.R.S.—4. An Enquiry concerning the Source of the Heat which is excited by Friction. By Benjamin Count of Rumford, F.R.S. M.R.I.A. (See Philof. Journal II. 106.)—5. Obfervations on the Foramina Thebefii of the Heart. By Mr. John Abernethy, F.R.S.—6. An Analyfis of the earthy

earthy Subſtance from New South Wales, called Sydneia, or Terra Auſtralis. By Charles Hatchett, Eſq. F.R.S. (See Philoſ. Journal, II. 72).—7. Abſtract of a Regiſter of the Barometer, Thermometer and Rain, at Lyndon in Rutland, for the year 1796. By Thomas Barker, Eſq.—8. An Account of ſome Endeavours to aſcertain a Standard of Weight and Meaſure. By Sir George Shuckburgh Evelyn, Bart. F.R.S. and A.S.—9. A New Method of computing the Value of a ſlowly converging Series, of which all the Terms are affirmative. By the Rev. John Hellins, F.R.S. and Vicar of Potter's-Pury in Northamptonſhire. And the Appendix, containing a Meteorological Journal kept at the Apartments of the Royal Society, by Order of the Preſident and Council.

Count Rumford's Experimental Eſſays, Political, Economical, and Philoſophical. Eſſay VII.—Of the Propagation of Heat in Fluids. Part II.—An Account of ſeveral New Experiments, with occaſional Remarks and Obſervations, and Conjectures reſpecting Chemical Affinity and Solution, and the Mechanical Principle of Animal Life. Octavo. 75 pages, with 2 plates. Cadell and Davies. Price 1s. 6d.

This ſecond part accompanies a new edition of the firſt. The philoſophical world will not need any general remark on the intereſting nature of the ſubject, nor the manner in which the great author has treated it. For the preſent, I copy the abridgment of its Contents, and ſhall ſpeedily give a fuller account.

Chap. I. Account of a circumſtance of a private nature, by which the author has been induced to add this and the following chapters to the ſecond edition of this Eſſay.—Experimental Inveſtigation of the ſubject continued.—Oil found by experiment to be a Non-conductor of heat.—Mercury is likewiſe a Non-conductor.—Probability that all Fluids are Non-conductors, and that this property is eſſential to fluidity.—The knowledge of that fact may be of great uſe in enabling us to form more juſt ideas with regard to the nature of thoſe mechanical operations which take place in chemical ſolutions and combinations; in the proceſs of vegetation; and in the various changes effected by the powers of life in the animal economy.—Rapidity of Solution no proof of the exiſtence of an attraction of affinity.—Strata of freſh water and of ſalt water may be made to repoſe on each other in actual contact, without mixing.—Probability that the water at the bottom of freſh lakes, that are very deep, may be actually ſalt - - - - - page 311

Chap. II. Water made to congeal at its under ſurface.—Obſervation reſpecting the formation of ice at the bottoms of rivers.—Reaſons for concluding that heat can never be equally diſtributed in any fluid.—Perpetual motions occaſioned in fluids by the unequal diſtribution of heat.—An inconceivably rapid ſucceſſion of colliſions among the integrant particles of fluids is occaſioned by the internal motions into which fluids are thrown in the propagation of heat.—An attempt to eſtimate the number of thoſe colliſions which take place in a given time.—Theſe inveſtigations will greatly change our ideas reſpecting the real ſtate of fluids apparently at reſt. Fluidity may be called the life of inanimate bodies. —Conjectures reſpecting the vital principle in living animals; and the nature of phyſical ſtimulation - - - - - - page 332

Chap. 3. Probability that intenſe heat frequently exiſts in the ſolitary particles of fluids, which neither the feeling nor the thermometer can detect.—The evaporation of ice during the

the severest frost explained on that supposition.—Probability that the metals would evaporate, when exposed to the action of the sun's rays, were they not good conductors of heat.—Mercury is actually found to evaporate under the mean temperature of the atmosphere. This fact is a striking proof that fluid mercury is a non-conductor of heat.—Probability that the heat generated by the rays of light is always the same in intensity; and that those effects which have been attributed to light ought perhaps in all cases to be ascribed to the action of the heat generated by them: a striking proof that the most intense heat does sometimes exist where we should not expect to find it.—Gold actually melted by the heat which exists in the air of the atmosphere, where there is no appearance of fire, or of any thing red-hot.—We ought to be cautious in attributing to the action of unknown powers, effects similar to those produced by the agency of heat.—The most intense heat may exist without leaving any visible traces of its existence behind it.—This important fact illustrated by the necessary result of an imaginary experiment - - page 345

Chap. IV. An account of a variety of miscellaneous experiments.—Thermometers with cylindrical bulbs may be used to show that liquids are non-conductors of heat.—Ice-cold water may be heated and made to boil standing on ice.—Remarkable appearances attending the thawing of ice, and the melting of tallow, and of bees-wax, by means of the radiant heat projected downwards by a red-hot bullet.—Beautiful crystals of sea-salt formed in brine standing on mercury.—Olive-oil soon rendered colourless by exposure to the air standing on brine.—An attempt to cause radiant heat from a red-hot iron bullet to descend in oil.—Account of an artificial atmosphere, in which horizontal currents were produced by heat.—Conjectures respecting the proximate causes of the winds - page 367

A Practical Essay on the Club-Foot, and other Distortions in the Legs and Feet of Children, intended to shew under what Circumstances they are curable or otherwise; with thirty-one Cases, and the Specification of a Patent granted the Author for his Method of Practice. By T. Sheldrake, Truss-maker to the Westminster Hospital and Mary-le-bone Infirmary. 8vo. 214 pages, with 14 plates. London: Printed for Murray and Highley, 1798.

This author shews by reasoning and induction, that the deformity which forms the chief subject of his treatise is produced by an unfavourable compression during the growth of the fœtus in utero. He adduces facts to prove, that mechanical means, if judiciously applied, will in numerous instances restore or place the organ in its proper state; and that it is of great importance that these remedies should be used as early as possible before the process of ossification is considerably advanced or completed. The contrivance and application of these means necessarily demand a knowledge of the formation of the parts, as well as of the nature of mechanical instruments. In both respects the author appears to have displayed considerable judgment, and has been rewarded by success. It is impossible to enter into any detail or description of his method within our limits. I shall therefore only say, that the nature of the cases and the respectability of the testimonies appear to deserve the attention of all who from professional pursuits or individual misfortune are urged to the consideration of this subject.

Fig. 1.

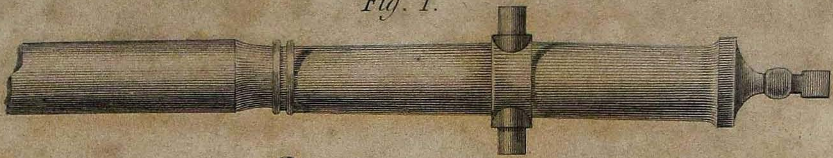
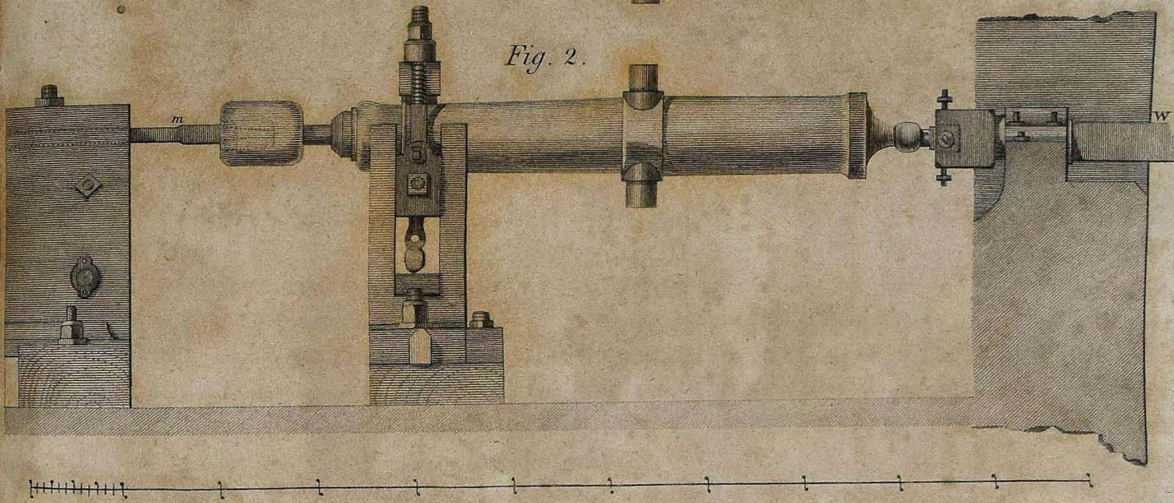


Fig. 2.



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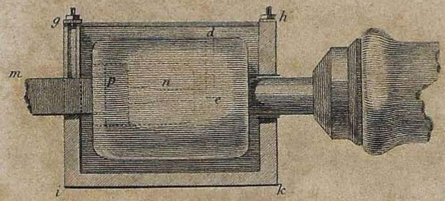

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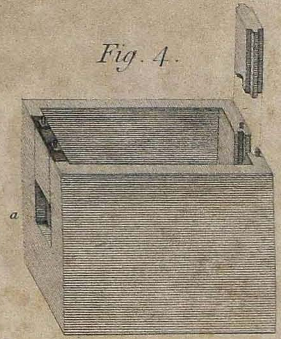

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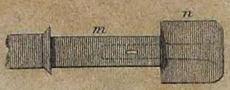

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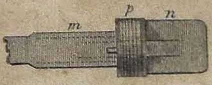

Barlow sculp.

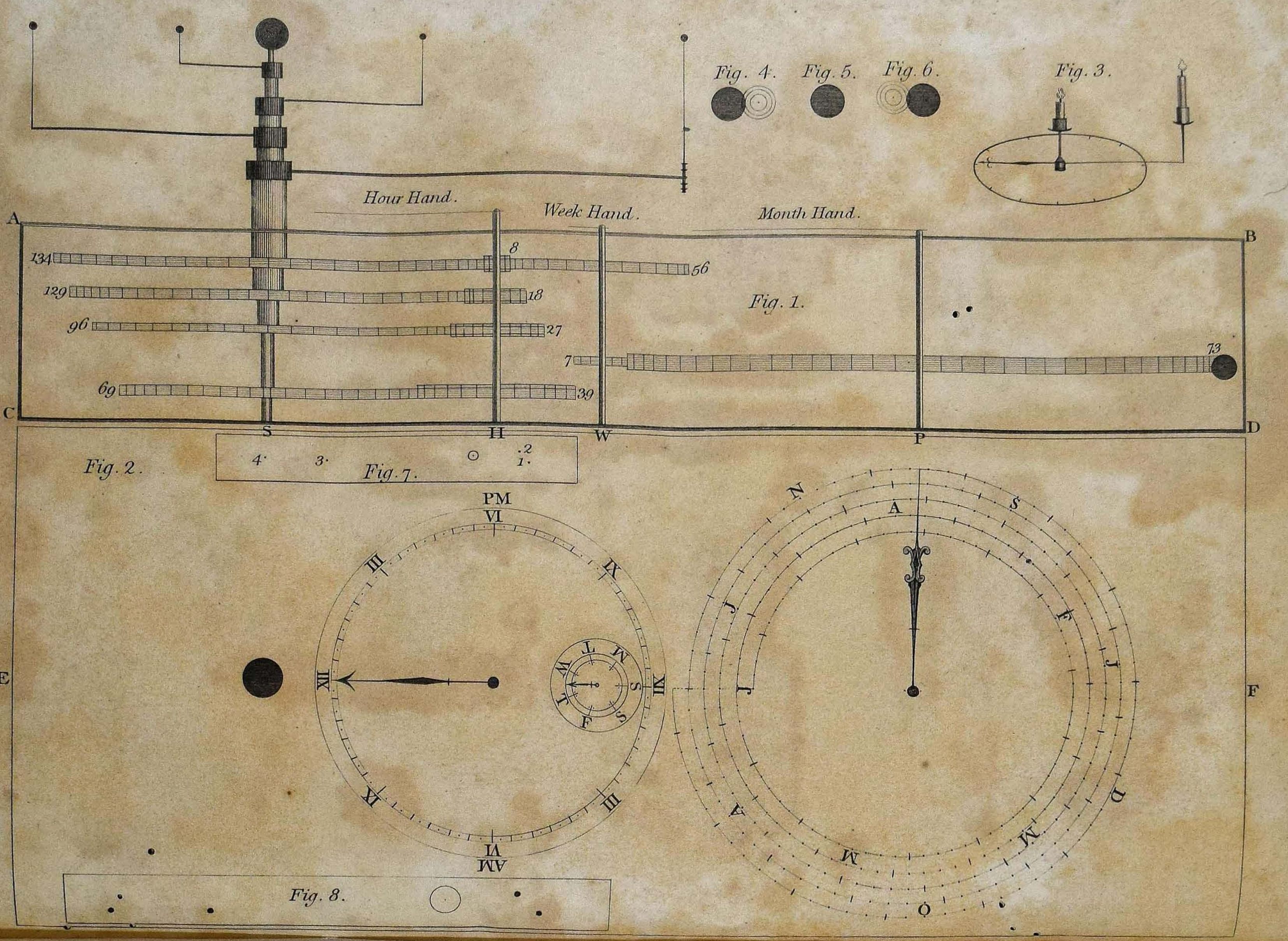
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Fig. 6.
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Fig. 8.